SCIENCE

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THE PRESENT STATE OF THE PROBLEM OF EVOLUTION[1]

The exchange of professors between the Sorbonne and Harvard University for the first time brings to Cambridge a professor of science. In a certain way I come in return for the visits which Professor M. Bôcher and Professor W. M. Davis have already made to the faculty of sciences at Paris. All my predecessors belonged to our faculty of letters. All have brought back a recollection of the hearty welcome which they received, and what they told me contributed largely in inducing me to accept the mission which was offered to me. I had the assurance of good-will and generous sympathy from my colleagues as well as from my pupils.

In the beginning I must excuse myself for not being able to express myself, at least for the present, in English. The most important point in teaching is clearness in expressing thoughts. By speaking to you in my own language I hope to succeed much better in a difficult subject and for that reason to obtain forgiveness for the effort which, to my great regret, I occasion you.

The purpose of the exchange between the two universities is to convey to the one the methods of teaching employed in the other. I have the honor to occupy at the University of Paris a chair of biology especially devoted to the study of the evolution of organic beings. It is then to the present state of this great problem that the lectures

[1] An introductory lecture in a course offered by M. M. Caullery as exchange professor at Harvard University, February 24, 1916. Translated from the French by Mrs. C. H. Grandgent.

which I am going to give will be dedicated. I do not enter upon this subject here without some apprehension. Certain of my predecessors, by the very nature of their subjects, were able to have, at least the illusion, that Europe is still the veritable center of learning. But I have not this advantage. The necessary conditions for the development of the sciences are now at least as well fulfilled, I will even say better fulfilled, in the United States than in Europe, and for many of the sciences, Europeans coming to this country have as much to learn as to teach. This seems to me particularly the case in biology and especially in the questions connected with the problem of evolution.

Besides, the advance of American science in these directions does not date from yesterday. In the study of paleontology, which has a large place in the questions with which we are to concern ourselves, your scholars have, for a long time, been working with activity and considerable success the marvellous layers of American deposits, and have drawn from them, to cite only one instance, magnificent collections of reptiles and mammals, which we come to admire in the museums on this side of the Atlantic. Here more than anywhere else have been enlarged the paths opened a century ago by George Cuvier. In zoology, properly speaking, the museum of comparative zoology, in which I have the honor to speak at this time, justly famous in Europe, bears witness to the importance and long standing of the results accomplished. Louis Agassiz, more than half a century ago, was one of the most eminent names of his generation. Later, when the investigation of the great depths of the ocean marked an important and consequent stage in the knowledge of earth and life, Alexander Agassiz, his son and illustrious successor, was one of the most eager and skillful workers. The expeditions of the *Blake* and of the *Albatross* are among those which have drawn from the deep the most important and most precious materials, and their results have been the most thoroughly studied. The personality of Alexander Agassiz, whom I had the honor of meeting in Paris about thirteen years ago, made upon me a striking impression. His real laboratory was the ocean, and he succeeded to the end of his life in maintaining an activity that corresponded to its amplitude. He was truly the naturalist of one of the great sides of nature. Around Louis and Alexander Agassiz, the museum and the laboratory of comparative zoology of Harvard College have been for a long time a center of studies of the first rank. In the domain of embryology Charles S. Minot also has carried on important work. But it is especially at the present moment that American biological science has made an amazing advance which expresses itself in the excellence of publications and in the results which they reveal by the number of collaborators, the activity of societies, the number of laboratories, and the abundance of material resources at their disposal. Here occurs a special factor, which has considerable importance, the enlightened and large generosity of numerous patrons. It is incontestable that men of talent find more easily in America than in Europe, and especially at the age of their full activity, the cooperation without which their greatest efforts are to a certain extent barren. Now, at the point to which we have arrived, the greater part of scientific problems demands the exercise of considerable pecuniary resources and of collaborators of various capabilities. This is particularly true of biology, where, moreover, many questions, notwithstanding their scientific importance, do not lead to practical application, at any rate immediately. We succeed too rarely in

Europe in combining these resources, above all in combining them rapidly enough. The European public does not sufficiently realize their necessity and interest. And the action of the state necessarily lacks the flexibility needful for rapid realization. Thus Pasteur was able to organize the institution which bears his name only at the end of his life, and at the inauguration he was heard to say mournfully, "I enter here defeated by Time." In America the power and the eagerness which private initiative gives provide for this need. Truly the greatest wonder is that this liberality is generally well conceived and well employed.

It is also true that the problems of the day in contemporaneous biology are nowhere else attacked at the present time with such activity, perseverance, and success as in the United States. As we look at different points on the biological horizon, we see the studies on the Mendelian theory of heredity in full development in numbers of laboratories. It will be enough for me to cite in this connection the names of Messrs. Castle and East in this very spot, and that of Mr. T. H. Morgan in New York. In the realm of the physiology and the structure of the cell and of the egg, the researches of E. B. Wilson, and of his pupils on the chromosomes, of J. Loeb on experimental parthenogenesis, of F. R. Lillie on the fertilization of the egg, of Calkins and recently of Woodruff on the senescence of the infusoria, suffice to show the share which this country has had in the advance of knowledge. And I ought also to mention numerous works on embryology and on the study of the filiation of the cells of the embryo (cell-lineage), on regeneration, on the behavior of the lower organisms, on geographic distribution and the variations of the species studied from the most diverse sides; all branches of biology are flourishing vigorously. In addition, the United States, more than any other country, has developed scientific institutions designed for the study of the application of biology to agriculture, to fisheries, etc.

In the face of this situation, I wish to make it clear at the outset that I have not the least expectation of bringing here a solution of the problem of evolution. I have too full a realization of the extent of the scientific movement aroused by this question in the United States and I hope to derive great benefit myself from my stay here, from the contact which is permitted me with my colleagues and with their laboratories. This latter advantage is not the least which arises from the exchange between the two universities. Nor have I the expectation of bringing to you a new solution of the problem, nor of examining it from a special and original point of view, such as might be the case in a single lecture or a small number of lectures.

I will adhere strictly to the point of view of the instructor, taking the question as a whole, expounding it in its older aspects as well as in its more recent ones. The interest in these lectures is above all, in my opinion, in the coordination of facts and in their critical examination. As this coordination is influenced in a large measure by the surrounding conditions, the view that a naturalist has of them in Paris ought to be interesting here. In questions as complicated and as undeveloped as these still are, where we have not reached a precise conclusion, the relations of facts can not be established in a harsh and unequivocal fashion. This is particularly true of the problem of evolution at the point we have reached. During the last few years very rapid and great progress has been made in our knowledge relative to certain kinds of data; notably heredity and variation. But they have not failed to shake markedly the notions which previously seemed to be

at the very foundation of evolution. One of my compatriots, an ardent disciple of Lamarck, F. Le Dantec, wrote even as far back as eight years ago a book bearing the significant title "La Crise du Transformisme"[2] in which he brought out the contradictions in question, contradictions which, according to him, were to result in the ruin of the very idea of transformism. Since that time opposition has become even more marked and at the present day, either tacitly or explicitly, certain of the most authoritative men, by their works, have arrived very near to a conception which would be the negation of transformism rather than its affirmation.

The term "evolution," in French at least, has had historically two contrary meanings. In the eighteenth century, it was the expression of the theory of the preformation or "emboitement" of the germs, according to which the lot of every organism was determined from the beginning. The succession of generations was only the unfolding (*evolutio*) of parts that existed from the beginning. In the nineteenth century, and it is in this sense that it is always used now, it had an opposite sense; it is the synonym of transformism and it signifies the *successive* transformation of animal or vegetable organic types, not realized beforehand, in the course of the history of the earth, under the influence of external causes. Now, if one admits the general value of certain of the ideas recently expressed, evolution would be only the unfolding of a series of phases completely determined in the germs of primitive organisms. It is a reversion, under a modern form, to the idea which the word evolution represented in the eighteenth century. It is unnecessary to say that I use the word evolution in its nineteenth-century sense, which is synonymous with transformism. It is evident then that all is far from being clear in the present conception of transformism and that, in consequence, an exposition of its various aspects and an effort to coordinate them is not a useless thing in a course of lectures. Furthermore a comprehensive glance at the principal questions which we shall have to examine will make my meaning clear and will give me the chance to indicate the general plan of the course.

In spite of the contradictions to which I have just alluded, the reality of transformism as an accomplished fact is no longer seriously questioned. We can make the statement that, in the unanimous opinion of biologists, evolution, that is to say, the gradual differentiation of organisms from common ancestral forms, is the only rational and scientific explanation of the diversity of fossil and living beings. All the known facts come easily under this hypothesis. All morphology in its different aspects, comparative anatomy, embryology, paleontology, verifies it. By virtue of this same hypothesis, these different branches of morphology have made an enormous progress since Darwin's day. The significance of certain categories of facts, especially in the domain of embryology, may have been exaggerated. Scientific men have certainly overworked the idea that the development of the individual, or ontogeny, was an abridged repetition of phylogeny, that is to say, of the several states through which the species had passed, an idea which Haeckel raised to the fundamental law of biogenesis and which a whole generation of naturalists accepted almost as a dogma. Without doubt, ontogeny, in certain cases, shows incontestable traces of previous states, and for that reason embryology furnishes us with palpable proofs of evolution and with valuable information concerning the affinities of groups. But there can no

[2] "Nouvelle collection scientifique," Paris, Alcan.

longer be any question of systematically regarding individual development as a repetition of the history of the stock. This conclusion results from the very progress made under the inspiration received from this imaginary law, the law of biogenesis.

The first part of the course will be devoted then to the consideration of the general data which morphology furnishes toward the support of the idea of evolution. Thus we shall see what conception comparative anatomy, embryology and paleontology affords us of the way in which evolution is brought about, and within what limits we may hope to reconstruct it. Evolution is essentially a process which belongs to the past and even to a past extraordinarily distant. It is a reasonable supposition that evolution is going on to-day, but let us remember that nothing authorizes us to believe that what we may observe in the present epoch about organisms will necessarily explain the succession of their former states. Evolution is an irreversible process and one which has not progressed at a uniform rate. We must not then expect to verify necessarily by the present organisms all the facts disclosed by morphology. It follows in my opinion that morphological data may force upon us indirectly certain conclusions even though we should have no experimental proof of them in contemporary nature.

Because of this very limitation which I have just pointed out, much of the difficulty of the study of the mechanism of evolution arises and to this may be attributed many of the profound differences among naturalists on the subject of evolutionary mechanism. The second part of the course will be devoted to the examination and the criticism of the solutions that have been proposed.

In a general way, the study of the mechanism of evolution is that of the reciprocal influence of agents external to the organisms, on the one hand, and of the living substance, properly speaking, on the other hand. There are then, if you wish, the external factors which together constitute the environment, and the internal factors which are the specific properties of the organism. These two elements are very unequally accessible to us. The environment is susceptible of being analyzed with precision, at least as far as the present is concerned, and we can surmise it with enough probability as to preceding periods. We know very much less about living matter, and especially about the way in which its properties may have varied in the course of time. Hence one meets with two tendencies which have been encountered ever since the evolutionary question arose and which are still very definite and very contradictory in their effects on the general theories of evolution. One of those attributes a large share to the external factors and attempts to explain facts by physico-chemical actions which are directly accessible. The other sees in internal factors, in the intrinsic properties of the organism itself, preponderant if not exclusive agents.

The first tendency attracts us more because it gives a larger share to analysis, that is to say to the truly scientific method. The second flatters our ignorance with fallacious verbal explanations. It is open to the objections brought against vitalist conceptions; and when, as is the case of certain old and new theories, we come to restrict the effective rôle to internal factors alone, we may ask ourselves whether there is a really essential difference between conceptions of this nature and creationist ideas; between declaring that species have been created successively and arbitrarily by an arbitrary sovereign will, without the external world having influenced their structure, or maintaining that organic forms

succeed one another, derived to be sure one from another, but following a succession that is really determined in advance and independent of external contingencies. Between such views there is in reality no considerable difference. Such an idea substitutes for successive creations one initial creation with successive and continuing manifestations. The present crisis of transformism, as Le Dantec and others set it forth, is the conflict concerning the reciprocal value of external and internal factors in evolution.

The two principal and classic solutions proposed to explain evolution were based on the efficacy of external factors, both the theory advanced by Lamarck in 1809 in his "Philosophie Zoologique," as well as that of Darwin formulated in 1859 in "The Origin of Species." Lamarck starts in fact with the statement that the structure of organisms is in harmony with the conditions under which they live and that it is adapted to these conditions. This adaptation is, in his opinion, not an *a priori* fact but a result. The organism is shaped by the environment; usage develops the organs in the individual; without usage they become atrophied. The modifications thus acquired are transmitted to posterity. Adaptation of individuals, inheritance of acquired characteristics, these are the fundamental principles of Lamarckism. Except for its verification, it is the most complete scientific theory of transformism which has been formulated, because it looks to the very cause of the change of organisms by its method of explaining adaptation. Darwin adopted the idea of Lamarck and admitted theoretically adaptation and the inheritance of acquired characteristics, but he accorded to them only a secondary importance in the accomplishment of evolution. The basis for him is the variability of organisms, a general characteristic whose mechanism he did not try to determine and which he accepts as a fact. This being so, the essential factor of the gradual transformation of species is the struggle for life between the individuals within each species and between the different species. The individuals which present advantageous variations under the conditions in which they live have more chance to survive and to reproduce themselves; those which on the contrary offer disadvantageous variations run more chance of being suppressed without reproducing themselves. There is established then automatically a choice between individuals, or, according to the accepted terminology, a *natural selection*, a choice which perpetuates the advantageous variations and eliminates the others. And with this going on in each generation the type is transformed little by little. Natural selection accumulates the results of variation.

This is not the time to discuss Darwin's theory. I wish only to observe to-day that it is less complete than that of Lamarck in that it does not try to discover the cause of variations; also that, like that of Lamarck, it attributes a considerable participation to the conditions outside the organism, since it is these finally which decide the fate of the variations. And one of the forms in which the opposition to the transformist ideas, at the time of Darwin, manifested itself, was the very argument that if organisms had varied it was only because of an internal principle, as Kölliker and Nägeli have more particularly explained.

The biologists at the end of the nineteenth century were divided with regard to the mechanism of evolution, into two principal groups, following either Lamarck or Darwin. Among the neo-lamarckians some have accorded to natural selection the value of a secondary factor, holding that the primary factors are the direct

modifying influences of the surroundings which according to them cause the variations. Selection came in only secondarily, by sorting out these variations and especially by eliminating some of them. Such was the particular doctrine developed by my master, A. Giard, at the Sorbonne. Others have more or less absolutely refused to grant any value to selection. Such was the case of the philosopher Herbert Spencer. We must also recognize that, since the time of Darwin, natural selection has remained a purely speculative idea and that no one has been able to show its efficacy in concrete indisputable examples.

The neo-darwinists, on their side, have, in a general way, gone further than Darwin because they see in selection the exclusive factor of evolution and deny all value to Lamarckian factors. This was the doctrine of Wallace, and has been especially that of Weismann. I will digress a moment to speak of the ideas of these last-mentioned authors, because of the influence which they have exerted and still exert, correctly in some respects, incorrectly in others, at least as I think.

Weismann attacked the doctrine of the inheritance of acquired characteristics and has incontestably shown the weakness of the facts which had been cited before his time in support of this kind of heredity. But he went too far when he tried to show the impossibility of this form of heredity. In so doing, he starts from a conception which meets with great favor; the radical distinction between the cells of the body proper, or *soma,* and of the reproductive elements or germ cells. He saw, in these two categories, distinct and independent entities, the one opposed to the other. *Soma* which constitutes the individual, properly speaking, is only the temporary and perishable envelope of the *germ* which is itself a cellular autonomous immortal line, which is continuous through successive generations, and forms the substratum of hereditary properties. The germ alone has some kind of absolute value. The *soma* is only an epiphenomenon, to use the language of philosophers. The *soma* is of course modified by external conditions, but for one to speak of the inheritance of acquired characteristics, the local modifications of the *soma* would have to be registered in the germ and reproduced in the same form in the *soma* of following generations, in the absence of the external cause which produced them in the first place. Now, says Weismann, the possibility of such an inscription, as it were, upon the germ of a modification undergone by the *soma* is not evident *a priori,* and when we go over the facts we find none supporting this conclusion. There are indeed modifications which appear in one generation and which are reproduced in the following generations; but Weismann goes on to attempt to prove that at their first appearance they were not the effect of external factors on the *soma,* but that they proceeded from the very constitution of the germ, that they were not really acquired and somatic, but were truly innate or germinal.

Such reduced to its essential points is the negative contention of the doctrine of Weismann. It rests upon the *absolute* and abstract distinction between the *soma* and the *germ.* In spite of the support which this conception has had and still has, I consider it, for my part, as unjustifiable in the degree of strictness which Weismann has attributed to it. It is true that the advance in embryology and cytology often allows us to identify the reproductive tissue and to follow it almost continuously through successive generations, but the conception of its autonomy is at least a physiological paradox. Though the continuity of the germ cells is sufficiently evident in many

organisms, it is more than doubtful in others, particularly in all those which reproduce asexually, that is to say, many large groups of animals like the Cœlenterata, the Bryozoa, the Tunicata, and many plants. This has more than the force of an exception, it is a general principle of the life of species. One can not then say that the conception of Weismann carries full conviction. But this conception exercised a tyrannical influence upon the minds of contemporaneous biologists and it is exclusively through it that most of them look at the facts.

Weismann, besides, exercised a considerable influence by championing a theory of heredity based at the start on the preceding ideas. This theory, built with undoubted ingenuity, and adapted to the knowledge gained from the study of cell division, turns out on the other hand to agree with the recent works on heredity.

Lamarckism and Darwinism shared the support of biologists up to the end of the nineteenth century, discussion being in general restricted to speculation. The controversy begun in 1891 between Weismann and Spencer, who represented the two extremes, gives an idea of the extent to which one could go in this direction.

The last twenty years constitute indisputably a new period in the history of transformism where the field of discussion has been renewed and scientists have sought to give it a much more positive and experimental character. Two kinds of investigation have been developed in this direction: on one hand the methodical study of variations, and on the other that of heredity and especially of hybridization. These two categories overlap.

Note that this new point of view is not, properly speaking, a study of evolution. According to it, variation and heredity in themselves, under present conditions, are analyzed independently of all hypothetical previous states of the organism. Afterwards the results obtained with the Lamarckian, Darwinian and other succeeding theories will be confronted.

The sum of these researches, which are now in high favor, is a new and important branch of biology, which has received the name of *genetics*. It defines for us in particular the hitherto very vague notion of heredity and seems certain to lead us to an analysis of the properties of living substance somewhat comparable to that which the atomic theory has afforded concerning organic chemistry. We can not maintain too strongly its great importance. As far as the theory of evolution is concerned the results obtained up to this time have been rather disappointing. Taken together, the newly discovered facts have had a more or less destructive reverberation. In truth the results obtained do not agree with any of the general conceptions previously advanced and do not show us how evolution may have come about. They have a much greater tendency, if we look only to them, to suggest the idea of the absolute steadfastness of the species. We must evidently accept these facts such as they are. But what is their significance? On the one hand they are still limited, on the other hand as I have already indicated above, and as I shall try to show in the following lectures, the advances made by the study of heredity in organisms, at the present time and under the conditions in which we are placed, does not permit us to accept *ipso facto* the doctrine of heredity for all past time and under all circumstances.

To use a comparison which has only the force of a metaphor but which will make my thought clear, the biologist who studies heredity is very much like a mathematician who is studying a very complex function with the aid of partial differential equa-

tions and who tries to analyze the properties and the function about a point without being able as in the case of an elementary function to study it in itself, directly, in all its aspects. The properties ascertained about one point are not necessarily applicable to all space.

As far as the organisms are concerned, the conditions of their variability have not certainly been the same in all periods. The idea of a progressive diminution of their variability has been often expressed, notably by D. Rosa. Le Dantec, according to his favorite theoretical method in which he considers only the fundamental principles of the problem, has tried to reconcile these facts with the Lamarckian doctrine in his book on La Stabilité de la Vie.[3] In the transformation of organisms as well as in that of inert matter, he regards every change as the passage from a less stable to a more stable state. The many organisms, after having varied much and rapidly, might then, perhaps, be for the present in a state of very constant stability, at least the greater part of them. But for the time being, I must omit further consideration of this suggestion.

We shall have then in the third part of the course to examine, while bearing in mind the preceding opinions, the general results of recent researches in variation and heredity. I shall now sum up the principal lines of investigation preparatory to tracing the plan of these lectures.

The methodical study of variations in animals and in plants has led us to recognize that the greater part of these variations are not inherited. If we apply to them the methods of the Belgian statistician Quetelet, we shall perceive that for each property numerically stated the different individuals of a species range themselves according to the curve of the probability of error, the greatest number of individuals corresponding to a certain measure which represents what is called the mean. The term *fluctuation* is given to those variations that are on either side of the mean and the study of these fluctuations, begun in England by Galton, has been developed and systematized by H. de Vries and Johannsen.

In short, it is the whole of the curve of fluctuations which is characteristic of heredity in a given organism, and not such and such a particular measure corresponding to a point in the curve. In cross-bred organisms there is, in each generation, an intermixture of two very complex inheritances, since these organisms result from an infinite number of these intermixtures in former generations. On the contrary, the problem is very simplified, if one considers the organisms regularly reproducing themselves by self-fertilization as is the case in certain plants. Here there is no longer in each generation a combination of new lines, but a continuation of one and the same line. It is the same hereditary substance which perpetuates itself. The Danish physiologist and botanist Johannsen attacked, as you know, the problem in this way, by studying variation along a series of generations in lines of beans, and the conclusion of his researches, which have had in recent years a very great influence is *that each pure line gives a curve of special fluctuations under special conditions.* The variations that we observe in the action of external agents explain the different reactions of the hereditary substance to the conditions of the environment, but this substance itself remains unaltered. The consequence is that, in what since the time of Linné we have considered a species, and have admitted to be a more or less real entity, there is an infinity of lines, more or less different among themselves in their

[3] "Bibliothèque scientifique internationale," Paris, Alcan.

hereditary properties, which are fixed and independent of environment. This it is that Johannsen calls the *biotype,* or *genotype;* a species is nothing but the sum of an infinity of genotypes differing very little from one another. H. de Vries on his side reached analogous views which prove to harmonize with the results and ideas formulated some forty years ago by a French botanist, Jordan, an unyielding adversary of transformism. Jordan, too, by means of well-ordered cultures, had analyzed a species of crucifer (*Draba verna*) in two hundred elementary species independent of one another. He deserves to be considered in any case as the precursor of the ideas of which I have just given a synopsis.

It is not then in ordinary variability, as it was known up to this time, that one can, following the ideas of De Vries and Johannsen, hope to find the key to evolution, since variations can not be the starting point for permanent changes. Examining a plant (*Œnothera lamarckiana*), De Vries thought he had found this key in abrupt transformations succeeding one another in organisms, under conditions which he has not been able to determine and which remain mysterious. The abrupt and immediately hereditary variations he named *mutations* and set them in opposition to *fluctuations* (*i. e.,* common variations). According to him, evolution is not continuous but operates through mutations. The theory of mutations has been, since 1901, the occasion of an enormous number of experimental studies and of controversies, into which I shall not enter at this time, but I shall finally endeavor to extract the results won by this method of work. Let us note that, if De Vries and the mutationists do not formally deny the intervention of external factors in the production of mutations, the rôle of these factors is no longer very clearly or directly apparent, and some deny it more or less fully. In short, systematic study has led to an antithesis between *fluctuations* produced under the influence of the environment but not hereditary, and *mutations* not directly dependent upon the environment but upon heredity. We shall have to discuss the value of this distinction, the extent and the importance of mutations.

Another and very effective branch of research which has developed since 1900 and which dominates the study of biology just now, is the study of hybridization, which has led to the doctrine known as Mendelism. Sometimes the name *genetics* is specifically applied to it.

Toward 1860, the study of hybridization had led two botanists, the Austrian monk Gregor Mendel and the French botanist Naudin,[4] simultaneously but quite independently, to conceptions which did not particularly attract the attention of their contemporaries but which were brought to light again in 1900 and which then formed the starting point of very many and important investigations. The experimental study of Mendelian heredity has been carried on, especially here in Harvard, with great success by Mr. Castle on various mammals, and by Mr. East on plants. This topic therefore is familiar to the students of biology in this university. I shall speak of it for the present, only to state the general results. Let me recall to your minds as briefly as possible the essentials of Mendelism; according to this doctrine most of the properties which we can distinguish in organisms are transmitted from one generation to another as distinct units. We are led to believe that they exist autonomously in the sexual elements or *gametes,* and we can, therefore by proper crossing, group

[4] "Nouvelles Recherches sur l'Hybridité dans les Végétaux." *Nouvelles Arch. du Mus. Hist. Nat.,* Paris, Tome 1, 1865, *cf.* p. 156.

such and such properties in a single individual, or on the contrary we can separate them. The biologist deals with these unit characteristics as the chemist does with atoms, or with lateral chains, in a complex organic compound. The properties which we distinguish thus are nothing but the very indirect external expression of constituent characteristics of the fundamental living substance of the species. But we imagine, and it is in this that the enormous importance of Mendelism consists, that it has been the means of giving us a more precise idea than we have had heretofore of a substantial basis for heredity. In itself, Mendelism is only symbolism, like the atomic theory in chemistry, but the case of chemistry shows what can be drawn from a well conceived symbolism and the Mendelian symbolism becomes more perfect each day in its form, in its conception and in its application. The recent works of T. H. Morgan[5] are particularly interesting in this respect.

Further, the facts furnished by Mendelism agree well with those of cytology. The results are explained easily enough, if we accord to the chromatine in the nucleus and particularly to chromosomes, a special value in heredity. The agreement of cytology and of Mendelism is incontestably a very convincing fact and a guide in present research.

But if we return now to the study of evolution, the data of Mendelism embarrass us also very considerably. All that it shows us in fact is the conservation of existing properties. Many variations which might have seemed to be new properties are simply traced to previously unobserved combinations of factors already existing. This has indeed seriously impaired the mutation theory of De Vries, the fundamental example of the *Œnothera lamarckiana* seeming to be not a special type of variation, but an example of complex hybridization. The authors who have especially studied Mendelian heredity find themselves obliged to attribute all the observed facts to combinations of already existing factors, or to the loss of factors, a conception which seems to me a natural consequence of the symbolism adopted, but which hardly satisfies the intelligence. In any case, we do not see in the facts emerging from the study of Mendelism, how evolution, in the sense that morphology suggests, can have come about. And it comes to pass that some of the biologists of greatest authority in the study of Mendelian heredity are led, with regard to evolution, either to more or less complete agnosticism, or to the expression of ideas quite opposed to those of the preceding generation; ideas which would almost take us back to creationism.

Lamarckism and Darwinism are equally affected by these views. The inheritance of acquired characters is condemned and natural selection declared unable to produce a lasting and progressive change in organisms. The facts of adaptation are explained by a previous realization of structures which are found secondarily in harmony with varied surroundings. That is the idea which different biologists have reached and which M. Cuenot in particular has developed systematically.[6]

Two recent and particularly significant examples of these two tendencies are furnished us by W. Bateson and by J. P. Lotzy. In his "Problems of Genetics," Bateson declares that we must recognize our almost entire ignorance of the processes

[5] *Cf.* Morgan, Sturtevant, Muller and Bridges, "The Mechanism of Mendelian Heredity," New York, 1915.

[6] Cuenot, "La Genèse des espèces animales," Paris, Bibliothèque Scientifique Internationale (Alcan), 1911.—"Théorie de la préadaptation," *Scientia*, Tome 16, p. 60, 1914.

of evolution, and in his presidential address at the meeting of the British Association in Australia, in 1914, he goes so far as to express the idea that evolution might be considered as the progressive unrolling of an initial complexity, containing, from the first, within itself, all the scope, the diversity and all the differentiation now presented by living beings. As Mr. Castle cleverly expressed it, carrying the idea to its logical issue, man might be regarded as a simplified ameba, a conclusion which may well give us pause. Here we clearly recognize, on the other hand, modernized in form, but identical in principle, the conception of the "enboitement" of the germs, and of preformation, ideas to which, as I have reminded you, the eighteenth century applied the name evolution. It is a conception diametrically opposed to that of the transformism of the nineteenth century.

Mr. Lotzy, struck by the results of the crossing of distinct species of *Antirrhinum*, has reached in the last three years the conclusion that a species is fixed and that crossing is the only source of production of new forms. Hybridization among species, when it yields fertile offspring, may, according to him, give rise, all at once, to a whole series of new forms, whose mutual relations and differential characteristics correspond exactly to what the natural species show.

However subversive and delusive ideas of this kind, positive or negative, appear to generations saturated with Lamarckism and Darwinism, we must not lose sight of the fact that they were formulated by eminent biologists, and that they are the result of long and minute experimental researches and that many of the facts on which they rest may be considered as firmly established.

But without thinking of rebelling against the facts resulting from genetic studies, we may question, whether they have so general a significance. I have already more than once pointed out that the present aspect of organic heredity does not oblige us to conclude that it has always been the same. We may ask ourselves whether conditions, which have not yet been realized in experiment, do not either modify directly the germinal substance itself, or the correlation existing between the parts of the *soma*, and indirectly through them the germinal substance. The facts which the study of internal secretions are just beginning to reveal, perhaps indicate a possibility of this kind. Even if we admit that evolution proceeds only discontinuously by mutations, we still have to discover the mechanism of the production of these mutations. In short, we may believe that, with heredity and variations acting as recent researches have shown them to act, there are nevertheless conditions that are still unknown and that they have been realized for each series of organisms only at certain periods, as seems to be suggested by paleontology, and in which the constitution and properties of hereditary substances are changeable. Of course these are purely hypothetical conjectures, but such conjectures must be made if we wish to reconcile two categories of already acquired data which we are obliged to recognize as facts. On the one hand we have the results of modern genetics which of themselves lead to conceptions of fixity, and on the other hand, the mass of morphological data which, considered from a rational point of view, seem to me to possess the value of stubborn facts in support of the transformist conception. I will even go so far as to say in support of a transformism more or less Lamarckian.

It seemed to me necessary to devote the first meeting of the course to this general analysis of the conditions under which the problem of transformism now presents

itself. I believe that this analysis is the justification of the course itself. It shows the advantage of confronting in a series of lectures the old classic data with the modern tendencies, all of which have to be brought into agreement. The crisis of transformism which Le Dantec announced some eight years ago is very much more acute and more in evidence now than it was then. In making this analysis, I have been able to furnish you in advance with an outline to the following lectures which together will form four successive parts; first, a rapid examination of data contributed to the support of the transformist conception by morphology in its different aspects (comparative anatomy, embryology, paleontology); second, the examination of the principal dynamic explanations of transformism, above all Darwinism and Lamarckism; third, a study of the main principles of genetics, and fourth, a few final lectures in which we shall review all the data.

A course on evolution might seem *a priori* a hypertrophy to a program of studies, and in fact it is nothing but an extremely restricted scheme for examining important questions and the many investigations which this line of study has brought forth. All I can do, then, is to confine myself to a general view of the question, limiting myself to facts and essential data.

M. Caullery

SIR CLEMENTS R. MARKHAM

Sir C. R. Markham, the famous geographer and explorer, who died in his London home, January 30, from burns caused by the overthrow of a candle, was in many respects a very remarkable man and his services to his fellows deserve to be widely known. He thought so little of himself that he did not trouble even to have a correct notice in "Who's Who in Science," nor did he talk or write of his own doings, so that, having survived most of his contemporaries, few were aware how much the modern world is indebted to him. Due to his sagacity and enterprise was the introduction of the quinine-producing shrub in India and the East; through his energetic work for twenty-five years as secretary to the Royal Geographical Society, and later as president, there was a vast increase in geographical knowledge and scientific exploration, whilst his published books on many diverse subjects were almost all on original ground. They would form an excellent course of study for any young man desirous to train mind and judgment on a good foundation. Each is a mine of careful research and accurate information, with utmost simplicity in presentation. There is no writing for effect and no self-exploitation; the narrative flows along easily and the reader can enjoy it as evidently the writer did.

Born in 1830, the son of the Vicar of Stillingfleet, Yorkshire, he entered the navy in 1844 and began his adventures hunting Riff pirates in the Mediterranean. In 1850, when the expedition in search of Sir John Franklin's party was preparing, he applied to join, and being refused on account of his youth, it is said that he sat down on the steps of the Admiralty and declined to move until the decision was reconsidered. Leaving in May, 1850, they returned in the autumn of 1851, having explored 300 miles of coast to about meridian 115 degrees on Melville Island, and in "Franklin's Footsteps" (published 1853), young Markham gave a spirited account of all they had seen. After wintering on Griffith Island, parties were sent in different directions over the ice, dragging by hand sledges with their limited provisions; McClintock's party covered 770 miles in 81 days, going 300 miles in a direct line from their ship. Markham was with a small party who went 140 miles in 19 days with one sledge. No wonder he spoke with genial scorn at a recent British Association meeting, of the modern polar explorer with every contrivance for comfort.

This expedition did not succeed in discovering the fate of Sir John Franklin and his crews, but it was one of twenty or more undertakings which between 1847 and 1857 went into the unknown north and turned the map of the Arctic circle from a blank into a well-charted region of desolate land and ice.

In 1852 Markham retired from the navy in order to travel and went to Peru, chiefly to study traces of the Incas. Reaching Lima from Panama, he rode on horseback along the coast south as far as Nasca, noting the wonderful Inca system of irrigation (the main trenches four feet high with roof and sides of stones) and then turned inland up to Ayacucho, and by Ollantaytambo to Cuzco. From there he went on by Paucartambo down the eastern side of the Andes and followed the course of the river Tono as far as its junction with the Purus. The Purus at that time was unexplored and there are other great blanks in the map made by Markham in 1859 for his Hakluyt edition of the early Spanish expeditions to the valley of the Amazons. In the course of this trip he learned Quichua, being at places where every one spoke that language, and he copied and studied the ancient native drama of Ollanta. Everywhere he got on well with the people, receiving hospitality in private houses, traveling with no luggage except saddlebags and making light of difficulties in the fashion of those days. He returned by sea from Islay to Lima. The pleasantly written "Cuzco and Lima" (1856) contains much general information and a chapter on Quichua grammar, a subject of study for the rest of his life. In the Introductory to the grammar and vocabulary published in 1864 he wrote:

> Ever since I was a midshipman on the Pacific station the land of the Incas has had for me an indescribable charm. I greedily devoured the pages of Prescott while anchored under the shadow of the mighty Cordilleras; and the story of the conquest and of the gentle children of the Sun, made an impression on me then, which time will never efface.

He goes on to say that, unable himself to continue the study of Quichua in the Andes, he hopes that his work may help others to make a start where he was obliged to leave off. In 1871 he published "Ollanta," from a good copy possessed by P. Justiniani (a descendant of the Incas), with an English translation. A revised translation forms the appendix to "Incas of Peru" (1910). He was assured by the natives in 1853 that the drama was undoubtedly composed before the Spaniards came:

> No European language can describe an action with anything like the precision and accuracy combined with brevity, of which Quichua is capable and the wonderfully abundant vocabulary produces great variety in composition.

This trip to Peru led to a second, for he had seen the shrub of the Peruvian bark and its use for malarial fever, and urged its introduction into India.[1] The Secretary of State for India consequently charged him with this important mission and in "Travels in Peru and India" (1862), he described his successful efforts. The Ecuador forest region he entrusted to R. Spruce,[2] who searched the western slopes of Chimborazo, took 1,000 cuttings and made a large number grow. He also collected seeds after carefully watching them ripen. Pritchett was told to collect in Huanuco and Markham himself reached Islay in March, 1860, and went first to Puno in the hope of interesting the Bolivian government but found opposition to any plan that might interfere with the local monopoly. He therefore went down to the Montaña over Peruvian ground by the Tambopata valley to Sandia in Caravaya, with Weir, a collector. There he found three desirable species, *C. micrantha* growing nearest the river, *C. Calisaya* in a zone above and *C. ovala* in a third zone, six to eight hundred feet above the river. He brought away 529 plants of six species, chiefly *C. Calisaya* and *C. morada*, returned safely by Lima and Panama to England and thence conveyed his precious cargo to India and the

[1] It was then excessively costly, as the collectors cut down the trees to get the bark.

[2] See Spruce's "Diary," edited by Alfred Wallace, 1908.

Neilgherry Hills. Successful production of quinine in India and distribution of seedlings all over Burmah and Ceylon reduced the cost so that it could be used by all. Of late years an artificial imitation has been used in the western world and can not be so beneficial. A similar tree with medicinal bark grows in Yucatan and at Cordoba in Mexico.

In the interesting preface to "Conquest of New Granada" (1912), Markham says that he found valuable drawings of the cinchona plants of Colombia by Mutis, in the toolhouse of the Botanic Garden at Madrid and obtained leave for their publication, edited by J. Triana.[3] He afterwards employed Cross to explore the region of *C. Pitayensis* in Colombia, east of Popayan and Timaná, and in 1867 published a translation of the works of Mutis and Karstan on the cinchona genus; also a handbook in Spanish for the use of cultivators.

Having joined the Royal Geographical Society in 1854, he became honorary secretary to the Hakluyt Society in 1858 and thenceforth some of his time was occupied in research for suitable manuscripts that he edited and in most cases translated from old Spanish, no small task. He was private secretary to the Under Secretary of State for India, 1862–64, and in 1865 was sent to Ceylon to report on the pearl fisheries. A military expedition to Abyssinia becoming necessary to rescue the British consul and other captives, Markham was sent as geographer, accompanying the troops to Magdala. In the "History of the Abyssinian Expedition" (1869), he gives admirable descriptions of the people, the geology and natural history of the land, with maps.

From 1863 to 1888, as secretary of the Royal Geographical Society, he had special opportunities to keep polar enterprises before the public. Through his indomitable energy the Nares expedition was fitted out in 1874 and he accompanied it as far as Greenland. He originated and promoted another expedition in 1875–76, when Commander D. Markham succeeded in reaching latitude 83 degrees, 20 minutes and 22 seconds, the highest northern position achieved up to that time. He also worked hard to obtain funds for Captain Scott's first expedition; the tragic end of Scott's party materially shortened his life. African exploration owed much to his encouragement of the pioneers who during the sixties endeavored to reach the sources of the Nile and discovered the great lakes, and at the British Association of 1864 he brought together Livingstone, Speke, Burton, Grant, Kirk and Sir Samuel Baker. Through him the Royal Geographical Society has been of untold benefit to scientific explorers by providing them with skilled instruction in nautical astronomy and surveying, etc. His address on the fiftieth anniversary of the society and the Review of Geography in his "Life of Major Rennell" (founder of modern geography), show the advance of the science until it has seemed to have no more worlds to conquer; for many years he was its inspiration and it took new force and meaning under his guidance.

The Hakluyt Society too has done excellently under him, bringing out many good editions of valuable old works of travel. In his address on the fiftieth anniversary in 1896, Sir Clements said:

> Our editors work gratuitously and for mere love of their authors. Every volume has an introduction and is annotated so as to give the reader all the help he can require in the study of the text.

Continuing with a graceful reference to the United States "whence we receive so much and such generous support," he added:

> The well-being of the Hakluyt Society is a symptom and not an insignificant one, of healthy tendencies of thought and healthy aspirations among the peoples who speak the English language.

The Founder's Medal of the Royal Geographical Society was bestowed on him in 1888; he was president from 1893 to 1906, remaining as vice-president to 1912; was also president of the International Geographical Congress, 1895–99; admitted as F.R.S., in

[3] "Nouvelles études sur les quinquina," Paris, 1870.

1873, and as F.S.A. about the same time and was president of the Hakluyt Society from 1889. In 1896 he was given the K.C.B., and other honors included a grand prix at the Paris Exposition of 1867 for the introduction of the cinchona cultivation into India; the order of the Rose from the emperor of Brazil, and of Christ from the king of Portugal. He married in 1857 a lady of the ancient family of Chichester. It is remarkable that the two branches of the family, both living in Devon, have never intermarried since the reign of Edward the Second, about 1310.

With Lady Markham he attended the meetings of the International Congress of Americanists at Stuttgart (1904) and Vienna (1908), and as president of the eighteenth congress (London, 1912), his support was most generous and energetic. He had hoped to take part in the nineteenth congress, held in Washington, December 27–31, 1915, for his heart was always drawn towards South American research, and he desired to aid it as far as possible. In a written message to members of the congress, he said:

> I regret extremely to be unable to attend, for I am deeply impressed with the great value and importance of these meetings. They are intended, as one main object, to supply to the minds of young explorers and students the best methods of obtaining accurate information and of using it when obtained. I think it should be impressed upon the rising generation of Americanists that study alone is insufficient for securing really satisfactory results; and that exploration and the collection of antiquities is not enough. The two branches must be combined. The study and use of authorities is by far the most difficult. In using them the character of the authority to be used must be carefully considered as well as his opportunities and his date. One great stumbling block for young students, whether in the study or in the field, is the adoption of a theory, leading to the search for its support. . . . A true worker should have no theory.
>
> I wish to submit my view to the congress that there is a splendid field for almost a life work in a study of the ancient civilization in the Peruvian coast valleys from Tumbez south. As yet it has not been touched by any one who is alike a diligent student with a profound knowledge of all that has been written in the past, together with the surveying, architectural and mechanical acquirements needed for a thorough examination of all that is to be found on the spot, and in museums. . . . I look upon a complete and thorough investigation of the history of the Chimu kingdom as one of the chief Americanists' desiderata.

Sir Clements Markham's life was full of achievements, such as would have been possible only to one fitted with extraordinary power and versatility. To have established in India and throughout the East, as he did, the cultivation of a prophylactic for the desolating malarial disease was a great service to humanity. Of boundless enthusiasm and tenacity of purpose, his ambitions were of the highest type, and his appreciation of the efforts of others to reach the points at which he aimed, was generous to the extreme. He was indeed a man in whom his countrymen could discern the best and most sterling qualities of their race.

A. C. B.

PRINCIPAL CAUSES OF DEATH IN THE UNITED STATES

According to a preliminary announcement with reference to mortality in 1914, issued by Director Sam. L. Rogers, of the Bureau of the Census, Department of Commerce, and compiled by Mr. Richard C. Lappin, chief statistician for vital statistics, more than 30 per cent. of the 898,059 deaths reported for that year in the "registration area," which contained about two thirds of the population of the entire United States, were due to three causes—heart diseases, tuberculosis and pneumonia—and more than 60 per cent. to eleven causes—the three just named, together with Bright's disease and nephritis, cancer, diarrhea and enteritis, apoplexy, arterial diseases, diphtheria, diabetes and typhoid fever.

The deaths from heart diseases (organic diseases of the heart and endocarditis) in the registration area in 1914 numbered 99,534, or 150.8 per 100,000 population. The death or mortality rate from this cause shows a marked increase as compared with 1900, when it was only 123.1 per 100,000.

Tuberculosis in its various forms claimed 96,903 victims in 1914, of which number 84,366

died from tuberculosis of the lungs (including acute miliary tuberculosis). As a result of a more general understanding of the laws of health, the importance of fresh air, etc., due in part, no doubt, to the efforts of the various societies for the prevention of tuberculosis, there has been a most marked and gratifying decrease during recent years in the mortality from this scourge of civilization. In only a decade—from 1904 to 1914—the death rate from tuberculosis in all its forms fell from 200.7 to 146.8 per 100,000, the decline being continuous from year to year. This is a drop of more than 25 per cent. Prior to 1904 the rate had fluctuated, starting at 201.9 in 1900. Even yet, however, tuberculosis has the gruesome distinction of causing more deaths annually than any other form of bodily illness except heart diseases, and over 40 per cent. more than all external causes—accidents, homicides and suicides combined.

Pneumonia (including bronchopneumonia) was responsible for 83,804 deaths in the registration area in 1914, or 127 per 100,000—the lowest rate on record. The mortality rate from this disease, like that from tuberculosis, has shown a marked decline since 1900, when it was 180.5 per 100,000. Its fluctuations from year to year, however, have been pronounced, whereas the decline in the rate for tuberculosis has been nearly continuous.

The only remaining death rate higher than 100 per 100,000 in 1914 was that for Bright's disease and acute nephritis, 102.4. The total number of deaths due to these maladies in 1914 was 67,545, more than nine tenths of which were caused by Bright's disease and the remainder by acute nephritis. The mortality from these two causes increased from 89 per 100,000 in 1900 to 103.4 in 1905, since which year it has fluctuated somewhat.

Next in order of deadliness comes cancer and other malignant tumors, which filled 52,420 graves in 1914. Of these deaths, 19,889, or almost 38 per cent., resulted from cancers of the stomach and liver. The death rate from cancer has risen from 63 per 100,000 in 1900 to 79.4 in 1914. The increase has been almost continuous, there having been but two years—1906 and 1911—which showed a decline as compared with the years immediately preceding. It is possible that at least a part of this indicated increase is due to more accurate diagnoses and greater care on the part of physicians in making reports to registration officials.

Diarrhea and enteritis caused 52,407 deaths in 1914, or 79.4 per 100,000. This rate shows a marked falling off as compared with the rate for the preceding year, 90.2, and a very pronounced decline as compared with that for 1900, which was 133.2. Nearly five sixths of the total number of deaths charged to these causes in 1914 were of infants under 2 years of age.

Apoplexy was the cause of 51,272 deaths, or 77.7 per 100,000. The rate from this malady has increased gradually, with occasional slight declines, since 1900, when it stood at 67.5.

Arterial diseases of various kinds—atheroma, aneurism, etc.—caused 15,044 deaths, or 22.8 per 100,000, in the registration area.

No epidemic disease produced a death rate as high as 18 per 100,000 in 1914. The fatal cases of diphtheria and croup—which are classed together in the statistics, but practically all of which are of diphtheria—numbered 11,786, or 17.9 per 100,000, in that year, the rate having fallen from 43.3 in 1900. This decline of nearly 59 per cent. is relatively greater than that shown by any other important cause of death. The rate has not fallen continuously, but has fluctuated somewhat from year to year.

Diabetes was the cause of 10,666 deaths, or 16.2 per 100,000. The rate from this disease has risen almost continuously from year to year since 1900, when it was 9.7 per 100,000.

The mortality rate from typhoid fever has shown a most gratifying decline since 1900, having decreased from 35.9 per 100,000 in that year to 15.4 in 1914, or by 57 per cent. This decline has been almost as great, relatively, as that for diphtheria, and has been greater than that for any other principal cause of death. The total number of deaths due to typhoid fever in 1914 was 10,185. The marked decrease in the mortality from this disease gives emphatic testimony to the effectiveness of

present-day methods, not only of cure, but of prevention. The efficacy of improved water-supply and sewerage systems, of the campaign against the fly, and of other sanitary precautions is strikingly shown by the reduction of the typhoid mortality rate to the extent of more than five ninths in 14 years.

The principal epidemic maladies of childhood—whooping-cough, measles and scarlet fever—were together responsible for no fewer than 15,617 deaths of both adults and children, or 23.7 per 100,000, in the registration area in 1914, the rates for the three diseases separately being 10.3, 6.8 and 6.6, respectively. In 1913 measles caused a greater mortality than either of the other diseases, but in 1914 whooping-cough had first place. In every year since and including 1910, as well as in several preceding years, measles has caused a greater number of deaths than the much more dreaded scarlet fever. The mortality rates for all three of these diseases fluctuate greatly from year to year. The rates for measles and scarlet fever in 1914 were the lowest in 15 years, while that for whooping-cough was considerably above the lowest recorded rate for this disease, 6.5 in 1904, although far below the highest, 15.8 in 1903.

Deaths due to railway accidents and injuries totaled 7,062, or 10.7 per 100,000. This number includes fatalities resulting from collisions between railway trains and vehicles at grade crossings. The death rate from railway accidents and injuries is the lowest on record and shows a most marked and gratifying decline as compared with the rate for 1913, which was 13 per 100,000, and a still more pronounced drop from the average for the five-year period 1906–10, which was 15 per 100,000.

Deaths resulting from street-car accidents and injuries numbered 1,673, or 2.5 per 100,000. This rate, like that for railway fatalities, is the lowest on record and shows a material falling off as compared with 1913, when it was 3.2, and as compared with the average for the five-year period 1906–10, which was 3.7.

The number of suicides reported in 1914 was 10,933, or 16.6 per 100,000 population. Of this number, 3,286 accomplished self-destruction by the use of firearms, 3,000 by poison, 1,552 by hanging or strangulation, 1,419 by asphyxia, 658 by the use of knives or other cutting or piercing instruments, 619 by drowning, 225 by jumping from high places, 89 by crushing, and 85 by other methods.

SCIENTIFIC NOTES AND NEWS

A supper will be given by the Harvey Society in honor of Dr. William H. Welch following his lecture upon Medical Education before the society on April 29. The supper will be given in Sherry's ballroom.

Oscar T. Schultz, M.D., professor of bacteriology and pathology in the University of Nebraska College of Medicine, Omaha, has been made director of the Nelson Morris Memorial Institute for Medical Research, Chicago. Max Morse, Ph.D., assistant professor of biochemistry in the college, has been appointed associate in chemistry in the institute.

R. E. Coker, Ph.D. (Johns Hopkins, '06), for several years director of the United States Fisheries Station for pearl-mussel investigation at Fairport, Iowa, has been promoted to be head of the division of scientific inquiry in the Bureau of Fisheries at Washington.

Dr. D. H. Scott, F.R.S., professor of botany in the London Royal College of Science, has been elected a foreign member of the Royal Swedish Academy of Sciences, in succession to the late Count Solms-Laubach.

The Founder's medal of the Royal Geographical Society has been awarded to Lieutenant-Colonel P. H. Fawcett, for his explorations and surveys on the upper waters of the Amazon; and the Patron's medal to Captain F. M. Bailey, Indian Army, for his exploration of the Tsangpo-Dihang River in the hitherto almost unexplored country where it breaks through the Himalayas. The Murchison award has been made to Lieutenant-Colonel Whitlock, R.E., for his work in connection with the delimitation of the Yola-Chad boundary in 1903–5, and the Yola Cross River boundary in 1907–9; the Back award to Mr. Frank Wild, second in command of Sir Ernest Shackleton's transcontinental Antarctic Expe-

dition, for his long-continued services in the exploration of Australia; the Cuthbert Peek award to Mr. F. Kingdon Ward for his journeys in the frontier regions between China and Burma, and to assist him in the further exploration of those regions; the Gill Memorial to Lieutenant-Colonel E. M. Jack, R.E., for his service in the delimitation and demarcation of the Uganda-Congo boundary.

As has been noted here the Nichols medal, awarded by the New York Section of the American Chemical Society for the best original contribution to the publication of the society during the year 1915, was conferred upon Dr. Claude Silbert Hudson, of the Bureau of Chemistry, in recognition of his research in the field of organic chemistry, at the regular meeting of the section, in Rumford Hall, Chemists' Club, March 10, 1916. In presenting the medal Dr. T. B. Watson, chairman of the New York Section of the society, quoted the specific character of chemical research represented by the different awards of the William H. Nichols medal in past years:

1903—Agricultural chemistry, E. B. Voorhees.
1905—Rare earths, C. L. Parsons.
1906—Organic chemistry, M. T. Bogert.
1907—Analytical chemistry, H. B. Bishop.
1908—Chemical engineering, W. H. Walker.
1908—Physical chemistry, W. A. Noyes and H. C. P. Weber.
1909—Organic chemistry, L. H. Baekeland.
1911—Physical chemistry, M. A. Rosanoff and C. W. Easley.
1912—Organic chemistry, C. James.
1914—Organic chemistry, M. Gomberg.
1915—Physical chemistry, I. Langmuir.

THE New York *Medical Record* states that the trustees of the New York Medical College and Hospital for Women gave a luncheon at Delmonico's on April 8 in honor of the fiftieth anniversary of the graduation of Dr. Anna Manning Comfort, the only surviving member of the first class graduated from the college. Dr. Comfort and Mr. Jefferson Levy, one of the incorporators of the institution, were the guests of honor. At the commencement exercises of this first class addresses were made by Henry Ward Beecher, Peter Cooper, Horace Greeley, Lucretia Mott, Elizabeth Cady Stanton and Dr. Lozier, dean of the college. The endowment of a scholarship at the college, to be known as the Anna Manning Comfort scholarship, was announced at the luncheon.

DR. CARLOTTA J. MAURY will make a paleontological expedition to the Island of Santo Domingo to study the Tertiary paleontology and stratigraphy, making collections and sections. This work will be carried on by the Sarah Berliner endowment. Dr. Maury has also been appointed special lecturer in paleontologic research at Cornell University for 1916–1917 on the Sarah Berliner Foundation.

THE University of Notre Dame has conferred the Lætare medal this year on Dr. James J. Walsh, author of publications on the history of science.

WE learn from *Nature* that at the University of Cambridge the Smith's prizes are awarded to H. M. Garner, St. John's College, for two papers on orbital oscillations about the equilateral triangular configuration in the problem of three bodies, and to G. P. Thomson, Corpus Christi College, for four papers on aeroplane problems. A Rayleigh prize is awarded to W. M. Smart, Trinity College, for an essay on the libration of the Trojan planets.

DR. L. JOST, professor of botany, has been elected rector of the university at Strassburg.

AT its meeting held on April 12, the Rumford Committee of the American Academy of Arts and Sciences voted a grant of $100 in addition to a former appropriation to Professor Frederick Palmer, Jr., of Haverford College, in aid of his research on the properties of light of extremely short wave-length.

DR. CHARLES WEISMAN, of the United States Public Health Service, has been transferred to Pittsburgh, which is the new headquarters of the service for work on industrial hygiene.

W. F. HORTON, mining technologist of the Bureau of Mines, has resigned to accept services with a steel company.

DR. H. H. MITCHELL has been appointed epidemiologist to the Indiana State Board of Health.

A. W. RICHTER, dean of engineering at the Montana State College, was elected president of the Montana Society of Engineers at the annual meeting which took place at Helena on April 7 and 8.

THE 722d meeting of the Philosophical Society of Washington will be held at the Cosmos Club, on April 20, when the address of the evening will be by Dr. R. A. Millikan, of the University of Chicago, on "Some Recent Aspects of the Radiation Problem." Members of the American Physical Society are invited to attend this meeting, which will be followed by a social hour.

AT the regular monthly meeting of the Cosmos Club, Washington, held on April 10, Dr. W. T. Swingle delivered an address on "Impressions of a Visit to Japan."

DR. FREDERICK H. GETMAN delivered a lecture on the "Nature of the Chemical Elements" before the Science Club of Wellesley College on April 11.

PROFESSOR W. S. FRANKLIN delivered a lecture on "Electric Waves" before the department of Electrical Engineering of the University of Illinois on April 6. He also spoke before the Physics Club on "Some Mechanical Analogies in Electricity and Magnetism." Two other lectures were given by Professor Franklin, one on the "Curved Flight of a Baseball" and the other on "Bill's School and Mine."

PROFESSOR LIBERTY H. BAILEY, of Ithaca; Dr. Ernest Burnham, of Kalamazoo; President Kenyon L. Butterfield, of the Massachusetts Agricultural College, and Professor E. R. Groves, of the New Hampshire College, will deliver courses of lectures at the summer graduate school of the Association of American Agricultural Colleges and Experiment Stations which will be held this year at Amherst, Mass., at the Massachusetts Agricultural College.

THE anniversary meeting of the British Chemical Society was held on March 30, when Dr. Alexander Scott delivered his presidential address, entitled "Our Seventy-fifth Anniversary."

DR. WILLIAM PALMER BOLLES died at Santa Barbara, Cal., on March 18. He was professor of materia medica and botany in the Massachusetts College of Pharmacy from 1874 to 1884, and instructor in materia medica and therapeutics in the Harvard Medical School from 1880 to 1884. He was until his retirement in 1908 surgeon at the Boston City Hospital.

UNIVERSITY AND EDUCATIONAL NEWS

WITH the exception of chemistry, all the departments of the Johns Hopkins University will be transferred to Homewood by October, 1916. The Johns Hopkins Club has contracted to take over the Carroll House on the Homewood campus.

BY the will of the late Colonel E. A. Knox the New York Medical College and Hospital for Women receives a bequest of $5,000.

DR. E. D. BALL, director of the experiment station and school of agriculture of the Utah Agricultural College has resigned to take effect at the end of the present year. Dr. Ball plans to go back into entomological work. Dr. F. S. Harris, professor of agronomy, has been elected director of the experiment station, and Dr. G. R. Hill, professor of botany and plant pathology, director of the school of agriculture.

DR. NELLIS B. FOSTER, assistant professor of medicine at Cornell University Medical School, has accepted the appointment of professor of medicine at the University of Michigan, Ann Arbor.

DR. FRANK WORTHINGTON LYNCH, formerly associate professor of obstetrics at the University of Chicago, has been made full professor of obstetrics at the University of California, succeeding J. Morris Slemons, 1900, who has accepted a similar chair at Yale.

DR. ERNEST LAPLACE, who has been professor of surgery and clinical surgery in the Medico-Chirurgical College, Philadelphia, for the last twenty years, has accepted also the duties of professor of principles of surgery and clinical surgery held by the late Dr. Rodman.

GARRETT RYLAND, Ph.D. (Johns Hopkins, '98), has been made professor of chemistry at Richmond College, Richmond, Virginia.

MR. A. V. HILL, Humphrey Owen Jones lecturer in physical chemistry at the University of Cambridge, has been elected a fellow of King's College.

MR. F. P. WHITE, St. John's College, has been elected to an Isaac Newton studentship at the University of Cambridge.

PROFESSOR SIEGFRIED GARTEN, of Giessen, has been called to the chair of physiology at Leipzig as successor to Professor E. Hering.

DISCUSSION AND CORRESPONDENCE

THE CURRENT "DEFINITION" OF ENERGY

TO THE EDITOR OF SCIENCE: In a book review by Professor Millikan[1] the reviewer incidentally mentions the existing confusion in the use of the word "energy." In my judgment, Professor Millikan's remark is fully justified; for it is not only the writers of text-books, but scientific writers of the first rank who find themselves more or less entangled with the current definition of energy and the terminology to which the definition leads because the terminology is inconsistent with a logical use of the facts. Recent and present writers are not wholly to blame for this state of affairs for they have inherited a "definition" and a terminology from the pioneers in the science of thermodynamics that conflict with facts whose full significance was discovered only after the terms were introduced and their use established. Under such circumstances confusion is inevitable until the terminology is revised to fit the facts.

Many of our text-books on physics "define" energy as the "capacity of doing work" (Maxwell), as the "ability to do work," or, even as the "power of doing work." This last is particularly reprehensible, because "power," as used in physics, is the rate of doing work. As a matter of fact, even if work were a form of energy, none of these definitions would be an adequate "definition" of energy any more than a quart measure would be a definition of "space." Because heat is a form of energy it does not follow that "energy is heat," or, because our standard of mass is a piece of platinum that "matter is platinum." But the above definitions of energy are worse even than the above logical absurdities would indicate, for work, as may easily be seen, is not even a form of energy, like heat, but is in reality merely a phenomenon that accompanies its transfer or transformation. The reason why our unit of work is also our unit of energy is that all of our measurements of work are *energy-changes* involving transfers which may be *measured* by the work done *on* or *by* a body or system. The actual doing of work is always found to depend upon the existence of *energy differences;* and these *differences* are just as essential to the doing of work and the transfer of energy as the presence of energy itself. This fact, which is ignored in the above definitions, is expressed in a variety of ways by the second law of thermodynamics. "The capacity of doing work," if the words are to mean anything definite should be taken as referring to the "availability of energy"; and the *availibility* of a thing is not the *thing* available. In explaining work and energy, Professor Millikan states:[2]

> . . . it is obvious that they are not synonymous terms, for a body may possess energy and yet never apply it to the production of work. Work is done only when energy is *expended.*

If he had here used the word "transferred" instead of "expended" his statement would confirm what I have been endeavoring to present.

There is no more necessity for a "definition" of energy than there is for a definition of "matter." Both are known only by their characteristic phenomena; and these characteristics must serve to identify them and to differentiate them from each other. With the "units" of each, however, the case is quite different. They may be defined in terms of

[1] SCIENCE, October 2, 1914, p. 486.

[2] "Mechanics, Molecular Physics and Heat," p. 42.

any *constant*, suitable, measurable, characteristic, phenomena. We do not have to "define" space because we have units of volume, or extension because we make use of meters, yards and feet. Next to an ignorance of facts, the principal source of confusion in the case of energy arises from using *one* characteristic attribute, and that not a universal one, as a "definition" of energy. Through supposing the indefinable to *be* defined, even the most careful writers are led into inconsistencies and mis-statements. The result, to the alert and critical student, is "confusion worse confounded." It does not follow that because our unit of work furnishes a very convenient and definite unit of energy that it is possible to "define" energy, or that work is a kind of energy. There is only one fundamental and universal characteristic of energy which we can be sure holds true for all of its various forms and that is its conservation. Energy is conserved; and this, if merely regarded as a postulate, necessitates our recognizing that when one form of it disappears another form takes its place. Equivalents of both can not exist at the same time. Hence, if work is a kind, or form, of energy it must possess and exhibit this characteristic, that while it exists in the form of work some other form must cease to exist, and *vice versa*. It can not be too strongly insisted upon that the property, or attribute, of conservation necessarily excludes all processes not included under transference and transformation. Again, although energy changes may be measured in terms of work the principle of conservation applies only to the energy; and it becomes possible to prove this principle only through the existence of some *one universal* form of energy into which all other kinds may be transformed. For it is evident that if there is no universal form there must be for each form, or kind, some special means by which it may be identified as energy and its equivalent value measured; otherwise the "principle of conservation" is a mere delusion, or purely imaginary. But so far as is now known all forms of energy without exception are susceptible to transformation into heat, either directly or indirectly through work, and their energy values determined in terms of heat. Hence for the present, at least, heat may be regarded as the universal form of energy.

In order to establish, definitely, the relation between heat and energy let us consider for a moment Joule's classical experiments for the determination of the mechanical equivalent of heat. The potential energy of the elevated weights *disappeared* during their descent and produced a quantity of heat which was measured. Now, by the principle of conservation, potential energy could be imparted to the weights only by the *disappearance somewhere* of an equivalent, either of heat, or of some potential form of energy. In either case, the elevated weights represented energy that has been accounted for without counting work *as energy;* hence the work done in elevating them can not have been energy. Nor is it in the case of the descending weights; for the potential energy of the descending weights disappears as potential energy and reappears as heat. *Work is then,* it can be seen, *a kind of process by means of which energy is transferred and transformed.*

Doubtless many will find it difficult to understand how the unit of work can be a correct and convenient unit of energy and yet not *be* energy. A parallel case is found in the measurement of temperature. The indications of the thermometric substance are due to heat yet are not heat; they must be interpreted as *ratios,* and merely show the relation of the temperature measured to some temperature assumed as a standard. Likewise a standard energy state is assumed and the *change* in the energy of the system may be measured by the work done *on* or *by* the system, an *inverse* corresponding change taking place in some other body or system. From the fact that the ratio of the work unit to the heat unit (energy) is known, the energy *change* is readily obtained by applying the ratio.

Since in teaching, concise, definite statements are desirable whenever possible, the current, defective and misleading "definitions" might be replaced by short statements like the following:

All physical phenomena are *effects* attributed to a universal activity called energy.

Since energy is conserved, or constant in amount, all of our experimental observations of it are limited to the various effects due to its transfer and transformations.

The doing of work indicates the transfer of energy (Maxwell).

All spontaneous natural processes may be made to do work (Nernst).

Transformations of energy take place accompanied by, or during, transfer.

Since writers of text-books and other writers who necessarily depend more upon authority than upon their own investigations and interpretations can doubtless quote the necessary "good authority" for their principal statements, when such statements are questioned, they will pay but little attention to adverse criticism so long as they have the necessary authority for their statements. This being only natural and reasonable, the foregoing view regarding the use and misuse of the words "work" and "energy" shall also be supported by quoting the necessary "high authority," Professor Clerk-Maxwell. All of the following quotations will be taken from two of his well-known and justly prized books, "Theory of Heat," tenth edition, which will be referred to as T. of H., and his "Matter and Motion," which will be referred to as M. and M. In addition to being a scientific investigator and mathematician of the first rank Professor Maxwell possessed a remarkable ability as a scientific writer and expositor.

The use of the term energy, in a scientific sense, to express the quantity of work a body can do, was introduced by Dr. Young (T. of H.), p. 91.

Dr. Young wrote at a time when the conservation of energy was yet unthought of. Hence Professor Maxwell "inherited" the definition—did not originate it. The inconsistencies in the following excerpts may safely be attributed to the growth of the subject and the failure of the later parts to agree with the older parts. A considerable part of the growth of the subject was due to the labors of Professor Maxwell himself.

For the energy of a body may be defined as the capacity it has of doing work, and is measured by the quantity of work it can do (T. of H., p. 90).

Energy is the capacity of doing work (M. and M., p. 101).

Perhaps those writers who "define" energy are not so much to blame, after all! They have, at least, "good authority." There could be no exception taken to the first statement if it confined itself to the following: "The energy of a body may often be measured by its capacity of doing work," *i. e.*, to transfer its energy; but there is no warrant for the last sweeping generalization that "energy is the capacity of doing work." It is indeed a striking example of a very common human trait—a tendency to repeat current familiar phrases without critical examination. Everybody does it more or less. All that the facts which he presented warranted him in claiming was that the capacity of doing work *is due to energy*, or, that *one important characteristic* of energy is its capacity of doing work, *i. e.*, of bringing about its own transfer.

Here then we have two sets of quantities, one relating to work, the other to heat. . . .

Of these quantities work and heat are simply two forms of energy (T. of H., p. 194).

It should be noted here that work is spoken of as a "form of energy."

The potential energy of a material system is the capacity it has of doing work depending on other circumstances than the motion of the system (M. and M., p. 120).

The preceding excerpts are sufficient to show the influence of Dr. Young's definition of energy. Some quite different statements as to the relation of work and energy will now be given—evidently the result of Professor Maxwell's own study of the subject, but whose full significance he did not then realize, or live to complete.

Work, therefore, is a transference of energy from one system to another; the system which gives out energy is said to do work on the system which receives it, and the amount of energy given out by the first system is always exactly equal to that received by the second (M. and M., p. 104).

Now it is evident as soon as the attention is called to it that work can not, at the same time, be both energy and the transference of energy. If two statements are inconsistent, one, at least, must be abandoned. Let us see which.

A similar inconsistency, or contradiction, is found in two recent, excellent, text-books, both by the same author, who quotes freely from Maxwell. In one book we find that "*energy is the capacity for doing work,*" while in the other book it is stated that "work may now be defined as the act of *transferring energy* from one body or system to another." If we combine these two statements in one we find that energy is the capacity for transferring energy!

The conflict evidently arises from retaining the old definition of Dr. Young which was introduced before the principle of conservation was recognized. It should be abandoned as no longer applicable. (See discussion of Joule's experiment given above, and the conclusion derived from it.)

In order to show that the last excerpt from Maxwell is not a mere slip of the pen but a conclusion based on evidence two additional excerpts will be given.

> The process by which stress produces change of motion is called work, and, as we have already shown, work may be considered as the transferance of energy from one body or system to another (M. and M., p. 164).
>
> The transactions of the material universe appear to be conducted, as it were, on a system of credit. Each transaction consists of the transfer of so much credit or energy from one body to another. This act of transfer or payment is called work. The energy so transferred does not retain any character by which it can be identified when it passes from one form to another (M. and M., p. 166).

We have, then, a conflict of authority from the *same source* and we must, perforce, decide from the evidence and not on the authority, and that is decidedly in favor of the later and consistent view that work is a transference of energy and not a "form of energy." The authors of text-books have just as good authority, if they care to use it, for defining work as a process of transference of energy as they have for defining energy as "the capacity of doing work"; and by so doing can place themselves more nearly in touch with recent developments as to what constitutes the relation between work and energy.

We have had one "definition" of energy; the following statement, by way of contrast, might also be used as another.

> Hence, as we have said, we are acquainted with matter only as that which may have energy communicated to it from other matter, and which may, in its turn, communicate energy to other matter.
>
> Energy, on the other hand, we know only as that which in all natural phenomena is continually passing from one portion of matter to another (M. and M., p. 165).

This latter, and later, conception of energy seems, to my mind, a long step in advance over the conception of energy as the "capacity of doing work." In addition, it is in full accord with the later developments of our knowledge of energy and with the general principle of conservation.

If we *accept* the conservation of energy as an established *principle,* then we must accept the legitimate deductions from it or abandon it as a principle. It is plain that neither the view that energy is a capacity of doing work, nor the view that makes work a "form of energy" is consistent with considering work a *transference* of energy; and also that while the last view is consistent with the principle of conservation the other two are not. The consistent view, and to that extent at least, the true view is, so far as my knowledge goes, the personal contribution of Professor Maxwell. No earlier, or contemporary writer, so far as I know, and they are not numerous, makes such definite and specific generalized statements. His treatment of energy in "Matter and Motion" is a distinct advance over his treatment of it in his "Theory of Heat." No doubt that if he had lived a few years longer he would have renewed his study of energy and cleared up his apparent inconsistencies. His later years were devoted

mainly to his "Electromagnetic Theory of Light," "one of the most splendid monuments ever raised by the genius of a single individual." All of the early investigators in the theory of energy received a peculiar bias from the fact that the theory of energy was developed from the theory of work—the production of "useful work" being one of the most important problems in the life of nations as of men. Hence the statement that "energy is the capacity of doing work" was evidently received and accepted by scientific men before and during Maxwell's time as expressing an advanced scientific generalization; and even now, when not too critically examined, might pass as equivalent to the statement: Energy is the universal natural agency by means of which work is done. But while the former statement is logically weak and leads to ambiguities and contradictions the latter statement is perfectly definite, consistent with Maxwell's showing that work is a transference of energy and with that broad general principle, the conservation of energy.

M. M. GARVER

STATE COLLEGE, PA.

A PECULIAR BREED OF GOATS

TO THE EDITOR OF SCIENCE: There is a peculiar breed of goats raised in central and eastern Tennessee. When suddenly frightened the hind legs become stiff and the animal jumps along until it recovers and trots off normally or if greatly frightened the front legs become stiff also and the goat falls to the ground in a rigid condition. They have received the name of "stiff-legged" or "sensitive" goats.

The farmers in Tennessee prefer them because they do not jump fences. They are snow white and look like ordinary goats.

We are starting experiments to determine whether this is a dominant or recessive characteristic in comparison with a normal goat.

When this peculiar affliction first appeared I can not say, but it seems to be possessed by all the goats in the section named.

J. J. HOOPER

KENTUCKY STATE UNIVERSITY

SCIENTIFIC BOOKS

The Natural History of Hawaii: Being an Account of the Hawaiian People, the Geology and Geography of the Islands, and the Native and Introduced Plants and Animals of the Group. By WILLIAM ALANSON BRYAN, Professor of Zoology and Geology in the College of Hawaii. Honolulu, Hawaii, The Hawaiian Gazette Co., Ltd. 1915. Distributors, H. S. Crocker & Co., 565 Market Street, San Francisco; G. E. Stechert & Co., 151 West 22d Street, New York. Price $5.50.

In 1907 and 1908 the American Association for the Advancement of Science thought seriously of going to Hawaii in the near future for a summer meeting. Prominent citizens of Hawaii joined the association in anticipation of this visit, and invitations from Hawaiian institutions were received in number. The then governor of the Islands, Mr. Frear, called on the Permanent Secretary in Washington, and Professor W. A. Bryan, of the College of Hawaii, attended the Chicago and Dartmouth meetings of the association in 1908, urging the mid-Pacific meeting. But difficulties of transportation arose, and the plan was finally abandoned at least until some future date. Professor Bryan's effort, however, was not without result, since during his visit he gained his charming wife, and has now brought out his great book on the natural history of Hawaii, thus bringing the islands to the continental members of the association to console them for the abandonment of the Hawaiian meeting.

Practically alone among the great scientific societies in this country, the American Society of Naturalists has preserved in its title the old idea of natural history. The old natural history is still talked about and written about, while the old natural philosophy, so-called, has gone out. But the old-fashioned natural history books, with their great charm and interest to a large class of readers, are seldom published nowadays.

This book of Professor Bryan's, however, is a real natural history. It covers in its six hundred pages the whole field. Section I.,

with its seven chapters, treats of the Hawaiian people, telling of the coming of the Hawaiian race, the effect of the tranquil environment of the islands upon the people, discussing also their physical characteristics and their culture in a broad way. Section II., with fourteen chapters, treats of the geology, geography and topography of the islands, including two chapters on the world's greatest active volcano, Kilauea. Section III. gives a consideration of the flora of the islands, devoting one chapter to the plant life of the seashore and the lowlands, and the other to the vegetation of the high mountains. Section IV. is on agriculture and horticulture in Hawaii, with four chapters. All of this portion of the volume is included in what is termed "Book I." Book II., in the same volume, considers the animal life of the group and devotes seventeen chapters to its consideration.

Of course the field covered is so great that the author can not pretend to speak authoritatively on all points, but he has carefully studied the writings of the many experts who have written about the differing topics, and acknowledges the assistance of many naturalists. But all information has been sifted and studied by the author who for many years has led an active naturalist's life in the islands.

The volume is elaborately illustrated with half-tones from original photographs, and includes 117 full-page plates from 435 negatives. Many of the plates are extremely beautiful.

The characteristics of the people are admirably explained by their environment. They were preeminently an agricultural people, and the lack of domestic and wild animals prevented them from following the hunting and pastoral life. As a result, they settled in permanent villages usually along the coast. "Since there were no noxious insects, poisonous serpents or dangerous birds or beasts of prey, there was no occasion for the alertness and constant fear that so frequently makes life in a tropical country a never-ending strain if not an actual burden." An interesting paragraph on the medicine of the Hawaiians indicates a very considerable degree of medical and surgical skill. It is interesting to note further that boxing was perhaps their national game, and was regulated by certain rules, umpires being appointed, and the victor defending the ring against all comers.

What one notes all through the book is an extraordinary condition of the fauna and their place. Some of these changes have been due to the struggle for existence alone; others have occurred directly through the agency of man. As an example of this last, the sandalwood trade, beginning about 1792, resulted in the practical extermination of that valuable tree. As early as 1831 the trade was on the decline, and by 1856 the wood had become very scarce. Many trees and plants purposefully introduced have thrived in an extraordinary way, some of them in fact becoming important pests. The mesquite of the southwestern United States and Mexico, straggly, unimposing, although very useful, shrub-like tree that it is, becomes in Hawaii a rather imposing feature of the landscape; glorious specimens grow in some gardens of the city of Honolulu, and the large pods form one of the most important stock foods. On the other hand, the lantana weed, introduced for ornament, speedily became so abundant as to ruin the land over large stretches of the country, driving out every other plant. Inspired by their success in introducing beneficial insects to prey upon injurious insects, the Sugar Planters Association imported certain plant-feeding insects to kill off the lantana. This experiment, although very dangerous and never to be advised, was in this case apparently very successful, and the lantana, although still abundant, is no longer a serious pest.

It is interesting to note that there are no land snakes in Hawaii.

An interesting section deals with the whaling industry, since the Hawaiian Islands were in the center of this trade for many years, the industry reaching its height in 1852, thousands of native Hawaiians being employed as whalers.

The consideration of the birds of the islands is very full, although to the casual visitor there seem to be practically no birds. Although there are 125 or more species enumerated, not more than half a dozen can be seen

in the city of Honolulu, and all of these have been introduced. In glancing through the bird section, I note on page 326 the heading "The Legend of Maui and the Alae," and this reminds me to mention the fact that all through the book are scattered native legends which add greatly to its interest.

Reverting again to the extinction of native forms, the statement is made on page 333 that the island of Oahu can make the melancholy boast that it has a greater list of extinct birds, in proportion to the total number of species known from the island, than any other like area in the world.

One of the most attractive fields of natural history study in the islands is that of the fishes. Fish have always been one of the chief articles of animal food of the natives, and many strange and beautiful species abound in Hawaiian waters. The collection of native fishing apparatus in the Bishop Museum is a revelation to the modern fisherman. The natives caught fish in many most ingenious ways, and were expert in making a certain fish poison known as *holahola.* They were expert shark fishers in the olden times, and the use of human flesh as bait was in great vogue. The person to serve as bait was killed two or three days in advance of the anticipated fishing expedition. His flesh was then cut up, placed in a container and left exposed to the air to decompose. Interesting but grewsome! In walking through the markets of Honolulu to-day, the visitor from the States is able to recognize practically none of the fishes exposed. The fish fauna of Hawaii is isolated from that of other lands, although most of the common families of sea fish have local representatives, some of them excelling in flavor the species which exist elsewhere. One is greatly attracted by the "butterfly fish" on account of their bright colors.

The chapters on native and introduced insects are very interesting; and of course every naturalist knows the tremendous interest attaching to the land and fresh-water shells of the islands, and their weight in the discussion of evolutionary problems.

There seems to be at least one striking exception to the general rule which we have mentioned, of the easy adaptation of other forms of animal life to the Hawaiian climate, in the case of the eastern oyster, which has repeatedly been introduced, but which has never become acclimatized.

In the portion relating to sea life the book is especially interesting, and the story of the plants and animals from the coral reefs is fascinating.

Scientific men have been criticized frequently in the columns of SCIENCE for bad writing. The criticism can not hold for the author of this book, since it is written in a style which even the professor of English at Harvard would, I think, like to claim for his own. The writer of this note can not improve upon a sentence which has been used by Professor Vaughn MacCaughey in writing of this "Natural History of Hawaii": "It is a great guide book to the life of the tropical Pacific; it is encyclopedic in its wealth and precision of detail, and philosophic in its breadth of treatment."

L. O. HOWARD

Exercise in Education and Medicine. By R. TAIT MCKENZIE, B.A., M.D. Second Edition. W. B. Saunders Company. 1915.

Muscular exercise has played an important part in man's history whether considered from the standpoint of his health, growth and physical development, or his achievements and progress in civilization. As a branch of science, the application of exercise in education and medicine is in its infancy. The extravagant claims of dabblers and charlatans have done much to confuse the real issues and to retard progress.

Dr. McKenzie has made a valuable contribution to the subject by bringing together in this volume all the available material representing the present status of our knowledge concerning the application of muscular exercise in education and medicine. Since the appearance of the first edition four years ago, this book has been the chief reference work on the subject of exercise. The second edition has been completely revised and enlarged to include all the new material which represents the considerable progress made in the subject during the past four years.

The first chapter contains splendid definitions and a new classification of exercises of speed, effort and endurance. Chapters two to six are devoted to physiology of exercise; they contain the results of laboratory and clinical findings on the behavior of the muscles, heart, lungs, the organs of nutrition and excretion, and the nervous system during and after different forms of exercise; also, modifications produced by differences in age, sex and occupation.

The two chapters devoted to the effects of violent exercise on the heart are of particular interest at this time when the subject is the cause of widespread discussion by physicians, and educators, and giving much concern to the parents of boys and young men interested in athletics. After reviewing the literature on the subject and citing a number of cases from his own wide experience, Dr. McKenzie arrives at the following conclusion: "After the most severe strain one can seldom find any measurable injury in a week's time in a heart originally sound if the athlete has not passed thirty. It is in those unprepared for violent exercise, and especially when approaching middle life, that the danger of heart strain is most imminent."

A classification of athletic and gymnastic exercises and games on the basis of the regions of the body used; the demand on nerve control; the influence on pulse, blood pressure, and respiration; the physical characteristics cultivated; and the best age for practise should prove of great value to the individual and the practitioner in solving the problem of exercise for the sedentary man.

The remaining eleven chapters in Part I. treat in detail of the various systems of physical education in different countries, physical education and athletics in schools, colleges, municipal and philanthropic institutions, and the special methods applied to the training of the blind, deaf mute, and mental and moral defectives.

In Part II., the first three chapters treat of the application of exercise, massage, vibration, and passive exercise to pathological conditions. The remaining thirteen chapters deal with the treatment by exercise of flat-foot, club-foot, round back, stooped and uneven shoulders, scoliosis, abdominal weakness and hernia, visceroptosis and constipation, diseases of the respiratory and circulatory organs, obesity, nerve pain and exhaustion, tic, stammering, chorea, infantile paralysis and locomotor ataxia.

The author has succeeded admirably in presenting clearly the methods of diagnosis and treatment of the various abnormal conditions which may be improved or corrected by exercise, manipulation and massage. The critical discussion of the various methods advocated for the treatment of hernia, scoliosis, diseases of the circulatory and respiratory organs, and obesity, is particularly valuable because of the author's long and successful experience in the treatment of these conditions.

A large number of diagrams, line drawings and photographs illustrating physical defects, exercises and equipment add materially to the value of the book. This book fairly represents the present status of physical education and mechano-therapy; its use as a guide and reference work by educators, teachers, physicians and other scientists interested in the physical development and improvement of man should aid materially in placing exercise on a scientific basis.

George L. Meylan

Columbia University

Electrical Engineering. By Charles Proteus Steinmetz. Fourth edition. Entirely revised and reset. 368 pp., 194 illustrations. McGraw-Hill Book Co.

Since the appearance in 1901 of Steinmetz's "Theoretical Elements of Electrical Engineering" the art of electrical engineering has progressed so rapidly that four editions of the book have been necessary to keep it up to date. The present edition is not merely a reprint from former ones but has been thoroughly revised and rewritten. Some matter which appeared in former editions has been withdrawn and new matter has been added with the idea of preserving the unity of the book and at the same time making it representative of theory and practise as it exists to-day.

The text is divided into two parts, the first

on general theory and second on the application of this theory to particular types of apparatus. In the part on general theory we note the author using the crank diagram for vector represention of alternating quantities. This departure from his previous custom (use of the polar diagram) is not due to the conviction that the crank diagram is superior to the polar (in fact the author still thinks the polar diagram preferable) but the crank diagram is used to make the text conform with the recommendations of the Turin International Electrical Congress. This change in Steinmetz's notation will undoubtedly be appreciated by engineering students who, in so far as the writer knows, never were able to see the superiority of the polar diagram and who were always somewhat confused in reconciling the almost universally used crank diagram with Steinmetz's pet, the polar diagram.

The second part of the text on Special Apparatus is opened with a brief analysis of the scheme of classification used in presenting the various machines. While the author's classification may upset some of our present notions, the sense of it is at once apparent and it will surely come into favor in the future. The electrical machines discussed fall into one or the other of five broad classes, each class embracing all machines operating on a given principle, whether motor or generator. These classes are: Synchronous machines, direct current commutating machines, synchronous converters, alternating current transformers and induction machines.

Many readers of electrical literature have all of Steinmetz's books; certainly every one should have at least this elementary text on alternating current circuits and machines.

J. H. M.

Electrical Engineering. By T. C. BAILLIE. Vol. I. Cambridge, University Press: G. P. Putnam's Sons. Pp. 236, 131 illustrations.

This text, dealing in an elementary fashion with electric circuits, machines and measurements, is intended as the introductory volume of a series of electrical texts being published in the Cambridge Technical Series.

On reading the book nothing new is found, either in subject-matter or method of presentation. There are several other books to be had which cover the same ground in practically the same way.

The title of the book is apt to mislead one regarding its contents; it might more suitably be called an introduction to the subject of electrical engineering. The work covered in the text is ordinarily given in a technical school by the department of physics, as will be evident from a brief review of the contents. The chapters are entitled: Currents of Electricity, Magnetism, Current Measurement. Electromotive Force, Resistance Measurement, The Potentiometer, Batteries and Electric Light.

The subject-matter is logically presented and is fairly well illustrated by original diagrams and cuts of commercial apparatus. To the layman desiring a knowledge of some of the underlying principles of electrical engineering or to the student attacking the subject for the first time, the text would be very helpful.

J. H. M.

Electrical Instruments in Theory and Practise. By W. H. F. MURDOCH and W. A. OSCHWALD. The Macmillan Co. 366 pp., 164 illustrations. $2.75 net.

The writers of this excellent book evidently possess the two requisites for a successful text, mastery of the subject and the ability to express their ideas clearly. One is convinced on reading this book on meters that the authors have carefully considered the theory of the various instruments and have worked sufficiently with the meters themselves to grasp the errors which may occur and the ways in which they can best be eliminated. A very useful feature of the book consists of experimental data which is liberally given throughout to show how nearly the theory may be expected to agree with practise.

The first chapter gives a condensed history of the early attempts to measure electrical quantities; it serves well to give the student a proper appreciation of the modern metering devices.

The second chapter deals with damping and how it is obtained in meters. Permanent magnet instruments, iron core instruments, electrostatic meters, hot-wire meters and dynamometer meters each receive one chapter.

Watt-hour meters are discussed at some length; the errors in reading due to friction, short circuits, etc., are illustrated by experimental results. Magnetic testing apparatus is described and typical results given. The last chapter deals with the Wheatstone bridge, the Kelvin double bridge, and the potentiometer, for both continuous and alternating current circuits.

The writer knows of no book on electrical measuring instruments which is its equal in value to the advanced engineering student. A companion volume dealing with oscillographs, ondographs and other special devices is promised by the authors for the near future; it should receive a hearty welcome.

J. H. M.

SPECIAL ARTICLES

A NEW METHOD FOR THE GRAPHICAL SOLUTION OF ALGEBRAIC EQUATIONS

THE writer recently devised a graphical method for the solution of algebraic equations that seems to be of such general interest and importance as to be worthy of publication in this journal.

Let us consider first an equation of the type

$$f(u) \cdot f(x) + f(v) \cdot F(x) + f(y) = 0,$$

where $f(u)$, $f(v)$, $f(x)$ and $f(y)$ are the same or different functions of u, v, x and y. Construct a chart (shown in outline in Fig. 1) consisting of three vertical axes, P, Q and R, any convenient distance apart, intersected by a horizontal axis H. Along the right side of the axis P plot the calculated values of $f(x)$, positive values being laid off *upward* and negative values *downward* from the horizontal axis at a rate of A units per centimeter, A being so taken that the values of $f(x)$ likely to be met will fall within the limits of chart.

In a similar way lay off values of $F(x)$ along the left side of the axis R, positive values being measured off *upward* and negative values *downward* from the horizontal axis, at the rate of B units per centimeter, B being so taken that the values of $F(x)$ likely to be met will fall within the limits of the chart.

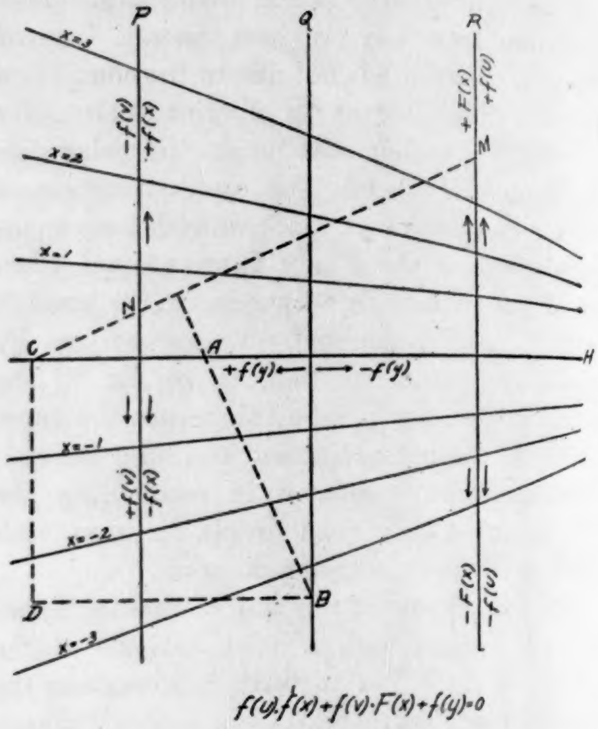

FIG. 1.

Along the horizontal axis lay off values of $f(y)$ at the rate of C units per centimeter, positive values of $f(y)$ being laid off to the *left* of the middle axis Q, and negative values of $f(y)$ to the *right* of that axis; C, being so taken that the values of $f(y)$ likely to be met will not lie too far to the right or to the left of the middle axis Q. Label the points thus located with the values of y used in calculating those of $f(y)$.

Values of $f(v)$ are to be laid off along the axis P in a way similar to that employed in laying off values of $f(x)$, positive values being measured *downward* and negative values *upward* from the horizontal axis H, at the rate of C/mB units per centimeter, where m is the perpendicular distance in centimeters between the outside axes P and R. Label the points thus located with the values of v to which they correspond.

In the same way, calculated values of $f(u)$ are to be laid off along the axis R, positive values being measured *upward*, and negative values *downward*, at the rate of C/mA units

per centimeter. Label the points thus located with corresponding values of u. To finish the construction of the chart, connect each value of $f(x)$ on the axis P with the corresponding value of $F(x)$ on the axis R by means of a straight line of indefinite length, which is labelled with the value of x to which it corresponds. In Fig. 1, several of such lines have been drawn and marked with the values $x=1$, $x=2$, etc.

Let it be supposed that the values of u, v and y in any particular example are known, and that the value of x is to be calculated. Locate the point M on the scale of $f(u)$ (axis R), marked with the given value of u. Locate the point N on the scale of $f(v)$ (axis P) marked with the given value of v. Connect M and N, and note the point of intersection, C, of this line or its prolongation with the horizontal axis H. Locate a point A on the scale of $f(y)$ corresponding to the given value of y. From A, draw a line perpendicular to the line MN, and note where its prolongation intercepts the middle axis at B. From B, draw a horizontal line, and from C a vertical line, intersecting at D. It will now be noted that D lies on a certain straight line, which is labelled with the value of x required; or it lies between two such lines, and the required value of x may be read by interpolation.

In practical work the chart shown in outline in Fig. 1 would be constructed on cross-section paper. We should need, in addition, a sheet of transparent paper or tracing cloth, having two perpendicular lines ruled on its surface. To solve an equation of the form we have been considering, simply move the transparent sheet back and forth over the chart until one of the two perpendicular lines appears to pass through the given value of u at M, and the given value of v at N, at the same time that the other perpendicular appears to pass through the point A corresponding to the given value of y. It will now be easy to note the apparent points of intersection of the perpendiculars with the vertical and horizontal axes at B and C respectively; and by following along the vertical and horizontal cross-sectioning from B and C we may locate the point D, and thus determine the required value of x, without actually drawing any construction lines on the chart itself.

As a special example of the use of such a chart, let us consider the calculation of the maximum temperature obtainable with natural gas burned without excess of air. The equation will be of the form $ax + bx^2 = c$, where the value of the coefficients a, b and c depend on the composition of the gas, and the specific heat at various temperatures of the products of combustion. In a particular case, let the equation be

$$3.2044\ t + 0.00074057\ t^2 = 8{,}203 \text{ calories.}[1]$$

The construction of the chart is simplified by the fact that the coefficients of t and t^2 to

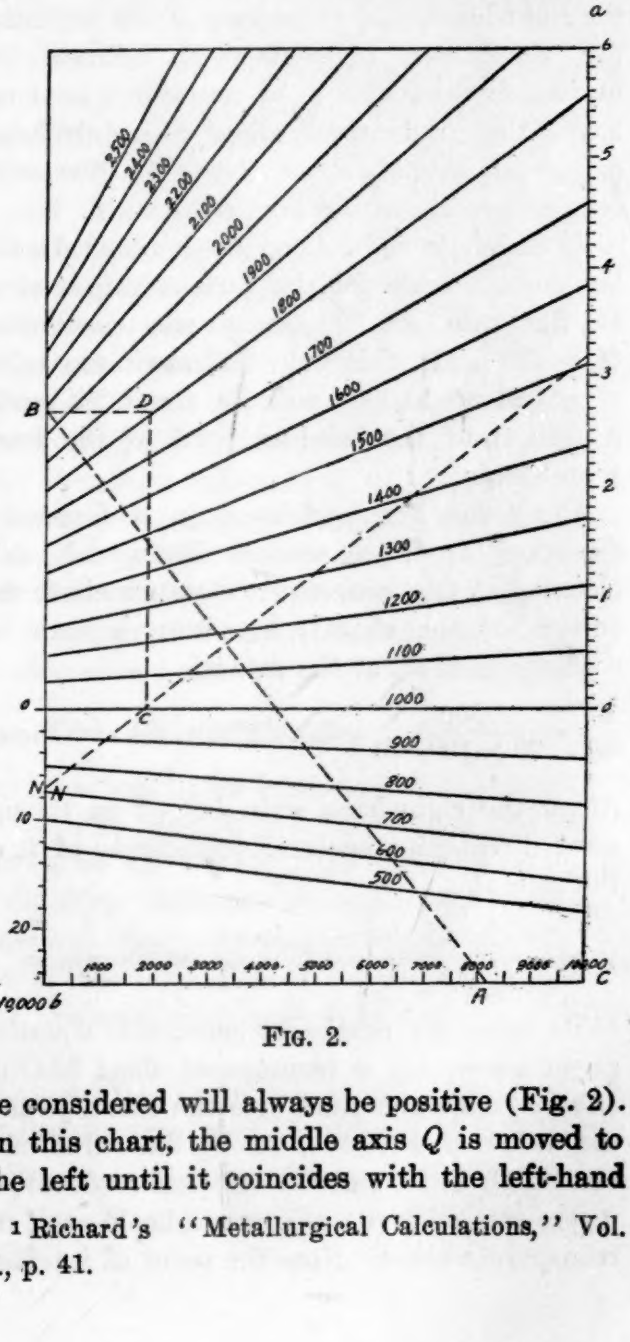

FIG. 2.

be considered will always be positive (Fig. 2). In this chart, the middle axis Q is moved to the left until it coincides with the left-hand

[1] Richard's "Metallurgical Calculations," Vol. I., p. 41.

axis, for the reason that the values of y (calories) to be considered will always be negative when transferred to the left-hand side of the equation, and will therefore lie to the right of Q. Since negative temperatures are not to be considered, the horizontal axis may be placed at the bottom of the chart.

In the construction of the chart let the vertical axes be taken 20 centimeters apart. Along the left-hand axis lay off values of t in an upward direction, at the rate of 100 units per centimeter ($A = 100$); in this way, the chart can be used for temperatures up to about 2500° C., if it be 30 centimeters high.

In a similar way, calculate the values of x^2 and lay them off in an upward direction along the right-hand axis, at the rate of 100,000 units per centimeter ($B = 100{,}000$). Since the maximum value of x to be considered is about 2,000, the graduations along the right-hand axis will extend about $2{,}000^2/100{,}000 = 40$ centimeters above the horizontal axis.

From left to right along the horizontal axis, lay down a scale for the various values of y, at the rate of 500 units per centimeter ($C = 500$). In this way, the maximum value of y (about 10,000) will lie about 20 centimeters from the left-hand end of the horizontal scale.

Along the left-hand axis, in a *downward* direction from a second horizontal axis (located at any convenient distance above the bottom of the chart), lay down a scale of coefficients of t^2, at the rate of

$$\frac{C}{mB} = \frac{500}{20 \times 100{,}000} = 0.00025 \text{ units per centimeter.}$$

Along the right-hand axis, lay off in an upward direction a scale of coefficients of t, at the rate of

$$\frac{C}{mA} = \frac{500}{20 \times 100} = 0.25 \text{ units per centimeter.}$$

To solve the particular quadratic equation given above, lay a transparent sheet bearing two perpendicular lines over the chart, so that the value of a (3.20), at M, the value of b (0.00074), at N, and the value of c (8203), at A, are crossed by the perpendicular lines of the transparent sheet. Note the point of intersection with the left-hand vertical axis at B, and with the auxiliary horizontal axis at C. From B and C follow along the horizontal and vertical cross-sectioning (not shown in Fig. 2) to locate the point D, where the required value of x (1805°) is read directly from the chart.

In Fig. 3 we have a further illustration of the use of such a chart in the solution of the equation

$$a \log x + b \sqrt{x} = c.$$

There are two values of $\sqrt{x}$, a positive and a negative one, for each value of x or $\log x$. There are accordingly two lines to be drawn from each value of $\log x$ on the left axis to connect with the corresponding values of $\sqrt{x}$ on the right axis. One of the two sets of lines thus formed has been shown in the figure by dashes.

Solution of particular equation

$$72.5 \log x + 6.25 \sqrt{x} = 54$$

is shown in the chart, the point D representing the value of x required. It will be noticed that in this case there are three different sets of lines that cross the region in which D happens to fall. There are accordingly three real roots to the given equation, the values read from the chart being 3.8, 10.5 and 530.

It is apparent that the number of real roots to any equation of the general form $a \log x + b\sqrt{x} = c$ will depend upon the values of the coefficients a, b and c. In the region in which the point D is shown, there are always three real roots, one of these satisfying the equation if a positive value of $\sqrt{x}$ be taken; the other two if a negative value of $\sqrt{x}$ be taken. In the region of the chart in which the point B falls, there is but one real root of the equation. If negative values of the coefficient b are considered the chart may need to be extended to the left of the left-hand axis; there will be two real roots in this region. If negative values of the coefficient a are considered the chart may need to be extended to the right of the right-hand axis. The trend of the lines in the diagram indicates that in this region there will in general be but one real root of the equation when a is negative; but in certain special instances, as for example,

when a is negative and c is very large, we may have two real roots; and there are other portions of the field where three real roots occur.

A use of the diagram, apart from the solving of equations, is thus to indicate the number of real roots that exist in the case of particular values of a, b and c. It is apparent that the chart will also indicate the effect of changes in the magnitude of the coefficients a, b and c on the absolute value or sign of x; and the reader will perceive that transcendental equations beyond the range of ordinary algebraic methods can be solved by this means.

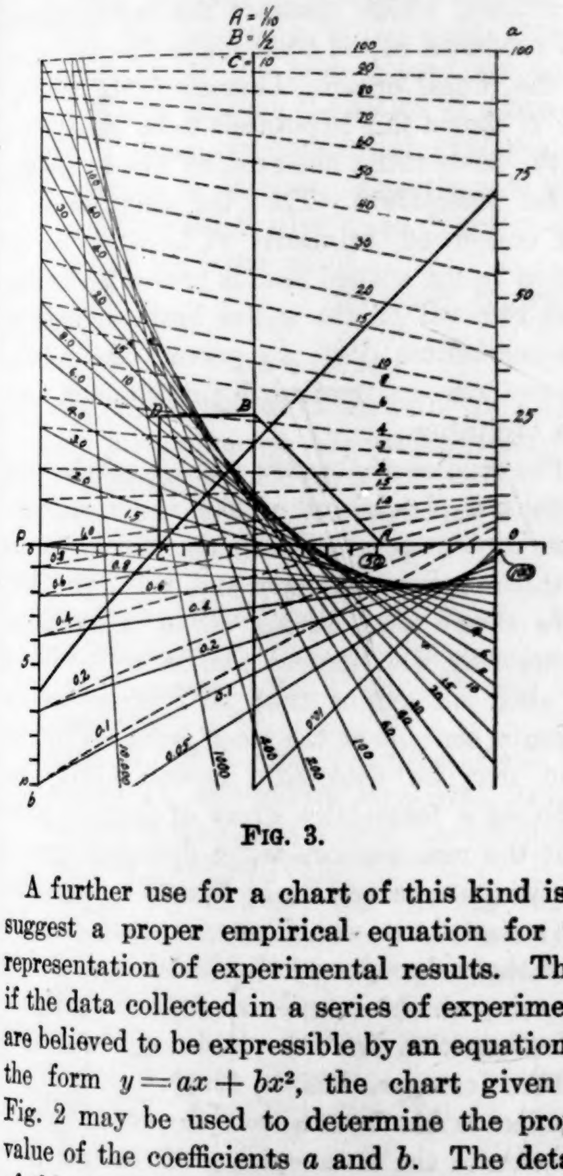

FIG. 3.

A further use for a chart of this kind is to suggest a proper empirical equation for the representation of experimental results. Thus, if the data collected in a series of experiments are believed to be expressible by an equation of the form $y = ax + bx^2$, the chart given in Fig. 2 may be used to determine the proper value of the coefficients a and b. The details of this procedure hardly require explanation; and other diagrams have already been published that constitute a graphical substitute for the method of least squares.

Returning now to the general case, it is evident that if $F(x) = 0$, we have

$$f(u) \cdot f(x) = f(y);$$

in this case the scale for $F(x)$ shrinks to a point at zero, through which all the lines representing different values of x must pass.

If $f(x)$ and $F(x)$ are constants the general equation takes the form

$$af(u) + bf(v) = f(y).$$

This may be charted as the so-called "alinement chart," well known to students of graphical mathematics.[2]

If $F(x)$ is replaced by $f(z)$ in the general equation we have five variables to consider. In this case $f(z)$ is plotted along the right-hand axis, the series of lines marked with the different values of x being omitted from the chart when the latter is first constructed. To use such a modified chart locate the point D in the usual way, then pass a straight line through D and that point on the right axis marked with the given value of z. The point of intersection of this line or its prolongation with the left axis gives the required value of x.

If two equations be given in which the values of x and z are to be determined, we locate two points D and D' in the usual way from the given values of u, v and y. Draw a straight line passing through D and D'. Its intersections with the left and right axes will give the values of x and z which simultaneously satisfy the equations. A set of three simultaneous equations of the general form

$$f(u) \cdot f(x) + f(v) \cdot f(z) + f(y) = 0$$

may be solved by an extension of this method.

It will be noticed that in the case last considered we are treating five variables, instead of the three that are included in the ordinary alinement chart. It was, indeed, by an extension of the principles of the alinement chart that the method presented in this paper was devised.

Exponential equations of all sorts may be handled by this method. Thus

2 See, for example, Peddle, "The Construction of Graphical Charts," New York, McGraw-Hill Book Co.

$$x^u \cdot y^v = z$$

can be put in the logarithmic form

$$u \log x + v \log y = \log z,$$

and charted immediately.

It is even possible to combine two or more charts of the general type we have considered, enabling us to solve equations containing four or more terms. The method is thus almost one capable of handling algebraic equations in general; but further development of the subject would be out of place here.

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THE COORDINATION OF CHROMATOPHORES BY HORMONES[1]

The melanophores of the horned toad, *Phrynosoma cornutum* Harlan, become contracted during states of nervous excitement. All attempts to prevent this reaction locally by cutting various nerves have failed. It is thus suggested that the melanophores may be coordinated, in part, by a hormone produced during nervous excitation and carried to all parts of the skin by the circulation.

The skin of one leg may be isolated from the general circulation, without blocking its nerve supply, by tying a ligature snugly about the leg. When this is done the melanophores of the isolated leg remain expanded after the animal is thrown into a state of nervous excitement. The leg appears much darker than its mate. Upon removing the ligature the melanophores contract and the leg becomes pale. The effect is not due to a shortage of oxygen or the accumulation of metabolic products in the leg, for such effects do not influence the melanophores of a ligatured leg until much later and then they produce a *contraction* of the pigment cells. If blood drawn from a horned toad which is in a state of nervous excitement is injected into one of the subcutaneous lymph-spaces of a second animal, the skin above the lymph-space will become very much paler than that of the rest of the body. The injection of blood from a horned toad which has not been thrown into a state of nervous excitement does not have this effect. During states of nervous excitement the blood contains a substance which causes the pigment cells to contract.

What is this substance and where is it produced? The conception of a hormone coordinating melanophore activity is not altogether novel, for Fuchs (1914, pp. 1546–1547, 1651–1652) has attempted to explain the behavior of pigment cells in amphibian larvæ and reptiles by assuming that substances, perhaps internal secretions, which contract the melanophores, are produced in the body under the regulation of the pineal organ. Laurens (1916) has recently shown this hypothesis to be inapplicable to the phenomena observed by him in *Amblystoma punctatum*. That the pineal organ is not concerned, primarily at least, in the reaction in the horned toad is proved by the fact that removal of the entire brain anterior to the cerebellum does not prevent the melanophores from contracting during states of nervous excitation.

The studies of Cannon and his collaborators upon the physiology of the major emotions present a more promising clue to the nature of this hormone. Cannon and de la Paz (1911) have shown that during states of emotional excitement the adrenal glands are activated to such an extent that an increase in the adrenin content of the blood from the adrenal vein may be detected. Spaeth (1916) has amassed a formidable array of facts to prove that the melanophore is "a disguised type of smooth muscle cell." If Spaeth's contention be accepted, it would appear most probable that the melanophores should be controlled by adrenin, which occupies a particularly significant position in the physiology of smooth muscle (compare Elliott, 1905).

Adrenin has been shown to produce a contraction of the melanophores of the frog (Lieben, 1906) and of *Fundulus* (Spaeth, 1916). Very minute quantities have this effect upon the melanophores of the horned toad. Removal of the adrenal glands does not prevent

[1] Contributions from the Zoological Laboratory of the Museum of Comparative Zoology at Harvard College, No. 273.

the reaction which follows nervous excitement. This fact, however, merely indicates that there are other mechanisms capable of bringing about the reaction. By stimulating the adrenal glands electrically the melanophores of the entire skin may be contracted. If one leg is ligatured during this procedure, it will remain much darker than its mate; if the ligature be removed several minutes after stimulation has been discontinued, the leg will quickly become as pale as the rest of the body. If the gland be isolated from the general circulation by a ligature, no contraction of the melanophores will follow the stimulation of its surface.

From the foregoing it is clear that the melanophores of the horned toad are coordinated, in part, through the action of a hormone. There is some circumstantial evidence that this hormone is adrenin. Experiments are in progress designed to give more direct evidence concerning the latter point.

ALFRED C. REDFIELD

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SOCIETIES AND ACADEMIES

THE AMERICAN PHILOSOPHICAL SOCIETY

ON March 3, Dr. Caroline Rumbold, University of Pennsylvania, spoke before the American Philosophical Society of Philadelphia on the "Pathological Anatomy of Injected Chestnut Trees."

While working on tree injection in connection with the chestnut-tree blight, about 50 different substances: hydrocarbons, alkali metals and metals were injected in solutions of varying dilution into the trunks of chestnut trees. So far, an examination of the trunks and branches of the trees shows that the reaction of the tree to the injections was alike in kind though not in intensity. This reaction varied with the distance from the point of injection. The affected region extended up and down the trunk from the point of injection in a line, whose width usually was but little more than the injection hole. As the distance from this point increased the tissues appeared more normal and the area of disturbance decreased. Occasionally all stages of reaction to an injection could be seen in a tree: death—at the point of injection—retarded growth, stimulated growth and no reaction.

The regions that showed response were the cambium and the phlœm. The cambium as such ceases growth and is wholly converted into wood-tissue. Small isolated groups of xylem cells develop on the outside of the rows of normal bast-fiber, through proliferation of the already formed phlœm cells. Large and very numerous stone-cells appear in the phlœm, which increase in number until rows of them are formed. An increased number of calcium oxylate crystals form. The isolated groups of xylem, developed in above-mentioned manner in the phlœm, grow in area and coalesce. In this conversion the cells of the phlœm take part with the exception of the bast-fibers and the stone-cells. They are frequently found embedded in xylem. This conversion proceeds irregularly, leaving areas of phlœm surrounded by xylem, or groups of cells of an undecided appearance, apparently partly phlœm, partly xylem. No specimens have been found in which all the phlœm cells in the injected region of the bark had been entirely converted into xylem.

The conversion of the cells of the phlœm into xylem cells is not unknown, but it is believed that this is the first instance in which by injected chemicals this phenomenon has been produced and it may prove a help in the future histological study of the cells of the phlœm.

THE BIOLOGICAL SOCIETY OF WASHINGTON

THE 552d regular meeting of the society was held in the Assembly Hall of the Cosmos Club, Saturday, March 11, 1916, called to order by President Hay at 8 P.M., with 28 persons present.

On recommendation of the council the following persons were elected to active membership: Dr. Molyneux L. Turner, R. T. Jackson, Biological Survey; H. L. Viereck, Biological Survey.

Under the heading Brief Notes and Exhibition of Specimens, Dr. Shufeldt exhibited lantern slide views of some of the aquatic and terrestrial vertebrates of the District of Columbia and Vicinity.

Under the same heading Mr. Wm. Palmer made remarks on and exhibited the bones of a hitherto unknown cetacean lately collected by him at Chesapeake Beach, Maryland.

The first paper of the regular program was by M. W. Lyon, Jr.: "Hemolysis and Complement Fixation." Dr. Lyon outlined the steps in the discovery of hemolysis by normal and immune serums from the early observation following transfusion by Landois in 1875, through Pfeiffer's phenomenon of bacteriolysis in 1889, Bordet's discovery of complement in 1899, Bordet and Gengou's discovery of complement fixation in 1901, to the practical application of the latter phenomenon as utilized by Wassermann in 1905 and by later workers in the diagnosis of syphilis, glanders, Malta fever, dourine, tuberculosis, infectious abortion, etc. The graphic conceptions of amboceptor, complement, antigen, and fixation as understood by Ehrlich, and as understood by Bordet, were illustrated by movable models. The action of hemolytic amboceptors and complement on blood cells of the ox and of the sheep was demonstrated by test-tube mixtures, and some positive and negative results in complement fixation were exhibited.

The last paper of the regular program was by D. L. Van Dine, "A Study of Malarial Mosquitoes in their Relation to Agriculture." Mr. Van Dine said: The Bureau of Entomology is making a study of the relation of malaria to agriculture and of the malaria-bearing mosquitoes, on a plantation in the lower Mississippi valley where typical conditions as regards malaria and plantation operations occur.

The object is to devise measures for prevention of malaria which will apply practically to farming conditions. Lines of work include determination of manner in which malaria operates in reducing farm profits, of the relative efficiency of *Anopheles* to act as transmitting agent and their distribution, of behavior of each species under known conditions of environment, and consideration of preventative measures which involve control of mosquito host.

Solution centers around prevention of malaria among tenants since it has been shown that the direct loss to planters occurs through lost time and reduced efficiency in labor. Detailed study was made of tenants, their relations to plantation, their habits and prevalence of malaria among them; the conclusion is that it will be more practical to control the mosquito than the human host.

One measure of prevention is favorable location of tenants' houses, demanding information on habits of flight, food and breeding. Where drainage is impracticable, surface water must be rendered unsuitable for *Anopheles* development. Food requirements and natural checks to larval development are being studied, the Bureau of Fisheries cooperating in a study of the relation of fish to mosquito development.

Anopheles quadrimaculatus, *A. punctipennis* and *A. crucians* were the species studied. *A. quadrimaculatus* is the common house-frequenting species of that region, *A. crucians* occurs in very limited numbers, and *A. punctipennis* is more restricted in its house habits but is common in nature. The work thus far has dealt almost entirely with *A. quadrimaculatus*, but following the demonstration of tertian and estivo-autumnal malaria in *A. punctipennis* by King in cooperation with Bass it will be expanded to include this species.

The study includes the habits of mosquitoes under low temperature conditions; also resistance of malaria organisms to low temperatures in body of mosquito host.

Mr. Van Dine's paper was illustrated with lantern slide views of the various conditions on the plantation. Messrs. Wm. Palmer, Doolittle and Knab took part in the discussion.

M. W. Lyon, Jr.,
Recording Secretary

THE BOTANICAL SOCIETY OF WASHINGTON

The 111th regular meeting of the Botanical Society of Washington was held in the Crystal Dining Room of the New Ebbitt Hotel, Washington, D. C., Wednesday evening, March 8, 1916. Eighty-two members and one hundred and seventeen guests were present. Professor A. S. Hitchcock presided. Dr. Rodney H. True, as retiring president, delivered an address to the society, entitled "Thomas Jefferson in Relation to Botany." This paper will be published in full in *The Scientific Monthly*. A dinner preceded the address and after it there was dancing.

W. E. Safford,
Corresponding Secretary